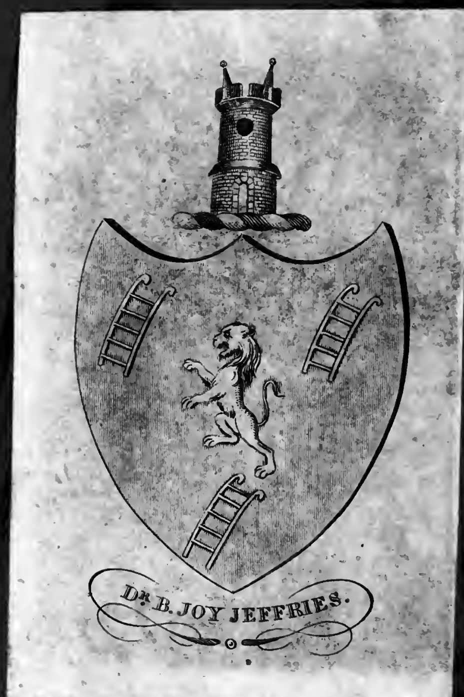
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SIGHT AND TOUCH.

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SIGHT AND TOUCH:

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AN

ATTEMPT TO DISPROVE

THE

RECEIVED (OR BERKELEIAN)

THEORY OF VISION.

BY

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ILLUSTRATED WITH WOODCUTS.

LONDON:

LONGMAN, GREEN, LONGMAN, ROBERTS, AND GREEN.

1864.

PREFACE.

The question discussed in the following pages is one of small compass, but no one versed in philosophy will estimate its importance by its extent. The theory assailed is in fact the stronghold of scepticism; for if consciousness is once proved to be delusive, there is an end to all appeals to its authority: doubt must reign supreme. It is to no purpose to say that it is not consciousness that is proved to be delusive, but an inveterate belief which is mistaken for a deliverance of consciousness; for it is practically the same thing whether consciousness itself deceives or something which is undistinguishable from it. It is of little use to prove that a certain witness is trustworthy, if in doing so we also prove that his evidence is falsified before it reaches us.

There is indeed only one dogmatic system consistent with the Berkeleian theory of Vision, and that is the Berkeleian Idealism. Yet dogmatists of all schools have accepted the theory. The only detailed attack upon it on philosophical grounds with which I am

acquainted is that of Mr. S. Bailey: "A Review of Berkeley's Theory of Vision, designed to show the unsoundness of that celebrated speculation" (London, 1842); and to this I owe some important suggestions. Sir David Brewster is known to be a decided opponent of the theory, but he has not attacked it on its philosophical side. I make no apology therefore for offering the following refutation, in which, as far as I was able, I have examined every aspect of the question.

Since these sheets were in press, the New Sydenham Society has issued the valuable work of Professor Donders, "On the Anomalies of Accommodation and Refraction of the Eye," which includes, in addition to his own important researches, all the essential information on the normal phenomena of adjustment and refraction, for which references had been given to foreign periodicals.

NOTE.—*References given at second-hand are enclosed in square brackets, and follow the name of the work from which they are taken.*

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SIGHT AND TOUCH.

INTRODUCTION.

If we were challenged to point out a single discovery in mental science which is universally admitted, we should at once name the "Theory of Vision" of Bishop Berkeley. Its success has been, indeed, extraordinary. From the time of its first publication it has been accepted almost without question. Adam Smith indeed whispered a doubt, and Sir W. Hamilton, with singular tenderness and timidity, insinuates that although the theory seems to be satisfactorily demonstrated, it is yet in certain circumstances, "provokingly found totally at fault." But the only decided assailant of the theory in recent times was Mr. S. Bailey, and he was at once set aside as an impracticable sceptic; Mr. J. S. Mill, who entered the lists against him, deeming apparently that in such a contest it needed but the very slightest effort of his skill to overthrow his adversary. At the present moment every one who has tasted philosophy, even *summis labris*, is firmly convinced that he sees, not persons and things of various bulk, and at divers distances, but merely a variety of colour, or at the best, a flat picture of no perceptible magnitude, and at no perceptible distance. Yet the profoundest metaphysician, when he opens his bodily eyes, is mastered by the same belief as the unlearned: he cannot see what he knows he does see, and he cannot help

seeing what he knows it is impossible to see. Seeing is, for the time, believing; but on deliberate reflection, sense is fairly overpowered by reason. This is truly a brilliant victory of science. An universal persuasion that nothing really exists, would be scarcely more surprising. It is an ungrateful task to attempt to deprive psychology of its only acknowledged triumph, and to wrest from philosophers the cherished analogy which never failed to support them in bidding defiance to probability and common sense. The adventurous critic who does so must encounter, not only the prejudice in favour of an established doctrine, but the not less powerful prepossession of logical minds in favour of what appears to be close reasoning. Yet this theory, whether true or false, is, in truth, the shame, not the glory, of psychology; for it is a discovery in its domain made, not only without its help, but in spite of it, by physical reasoning. It has hitherto baffled psychology, either to verify it or to shake the physical basis on which it rests. But if the closest attention fails to discriminate ideas, not only successive, but of diverse senses—fails to distinguish perception from recollection—fails to discover ideas known to be constantly present—if psychology has been forced to make this ignominious confession at its first encounter with physics, it is time that it should abandon all pretence to be a science of observation, or, indeed, a science at all. The present state of the question is, moreover, a disgrace to philosophy. The theory, we are told, is at variance with common facts, and yet the proof appears to be "as satisfactory as anything in the whole compass of inductive reasoning."* By exposing its fallacy, we shall not only remove a reproach from philosophy, but shall sweep away with it not a few erroneous doctrines, of which it has been the chief or sole bulwark.

* Hamilton on Reid, p. 183.

CHAPTER I.

THE PARADOX AND ITS PROOF—STATEMENT OF THE QUESTION.

LET us first consider the *prima facie* evidence of common sense. When I feel an object with my finger, there can be no sensation produced unless some force be exercised; for instance, by pressure, which is followed by an unknown change in the nerve of touch. In order to exert this force, the object must be within a very minute distance; and if that distance does not exceed a certain small limit, each sensibly distinct point of the object affects but one distinct sensitive spot. In this respect, sight differs from touch only in the much greater variety of distance at which an object can so affect the organ. In another respect, however, the two senses are psychologically contrasted, there being in the sense of sight less consciousness of ourselves as affected, than in any other sense. In some of the senses we are conscious of little but the affection of our organism; as in taste and smell, which are not accompanied with the perception of an external object. In touch we perceive something external, but are at the same time conscious of an affection known as the *feeling*, of hardness, &c.; in sight, on the contrary, we are not conscious of any feeling whatever. We see external things, their figure and colour; for though colour is in one sense a sensation, we are not conscious of it as such—red is perceived as red, in consequence of the constitution of our organs, but it is perceived as belonging to and in the place of the object seen. A striking testimony to this peculiarity of sight is found in the metaphorical use of terms borrowed from the different senses. Do we speak of knowledge, pure and simple, we use

the verb *to see;* while, on the contrary, *feeling* is in common speech synonymous with sensation. Now, what is the extent of this perception of sight? Ask the common man how he knows that objects exist without him, that they are of such and such a figure, that they are at a distance; he will at once answer—"Do I not see them?" "But how do you know that the tables and chairs you see are not an illusion?" To this he will doubtless reply by an appeal to the sense of touch and resistance. But it is easy to distinguish the sphere of the testimony of the two senses. For, again, let us ask the man who has *felt* a pressure or resistance, how he knows that the cause is external, and it is invariably to sight that he appeals. Thus, if touch assures us of the substantial reality (objective existence) of the objects of sense, sight gives the most decided testimony to their externality and distinctness from ourselves. In touch, moreover, we perceive something resisting—something, therefore, outside us, or rather, which is different from the feeling organ, yet at no distance from it, for outness and distance must not be confounded.* Indeed, most of the words which express apposition without interval are borrowed from touch. The object of sight, on the contrary, is perceived as at some distance: we can move towards it or away from it, or interpose our hand between it and the eye. This is the doctrine of common sense. To believe it is philosophical heresy; it is to prove one's self incapable of appreciating a fine distinction or a subtle argument. The orthodox doctrine, the discovery of which is Berkeley's chief claim to immortality, is the following.

We say, for instance, that we see an orange, which we can feel by putting out our hands, and if we choose, can divide and suck; we see, that is to say, its figure, size, colour, externality, and distance. In truth, says Berkeley, we see no such thing. By sight, we are aware of a yellow figure, which

* See Hamilton on Reid, p. 177.

however has no resemblance to the figure perceived by touch;* of a certain degree of dimness or clearness, distinctness or confusion, and of a certain sensation in the muscles of the eye. But these "visible ideas" being constantly associated with certain ideas of touch—as softness, roundness, distance, magnitude, and figure (which have no resemblance whatever to the former, nor any natural connexion with them)—these latter come to be immediately suggested by the former, and that so certainly and quickly, that we mistake them for objects of the same sense, and say that we see the figure, distance, magnitude, &c.; whereas these attributes and that of outness being implied in the idea of an orange, we do not really see the orange, except in the same sense in which we may be said to see "anger or shame in the looks of a man,"† or in which we "hear a rock,"‡ when the word is pronounced. "It is a mistake to suppose that we see and feel the same object."§ He "would fain be informed how we come to know that the same object causeth ideas by different senses."|| Berkeley, logically enough, went further than his successors in accounting in the same way for the idea of number given by sight. That we see one orange only, even with one eye, is, according to him, the result of association. In order, however, that I may not be charged with putting an interpretation upon the "inaccurate"¶ language of Berkeley, which his disciples would disown, I shall quote and use Mr. Mill's own statement, viz., "That the information obtained through the eye consists of two things—sensations, and inferences from those sensations: that the sensations are merely colours variously arranged, and changes of colour; that all else is inference, the work of the intellect, not of the

* "Theory of Vision," secs. 117, 127. † Ibid, sec. 65.
‡ Ibid, sec. 136. § Vindication, sec. 21. || Theory, sec. 108.
¶ Berkeley's language is really accurate enough, but is, of course, tinctured with his own idealist views.

eye."* "When we judge by the eye of the remoteness of any object we judge by signs; the signs being no other than those which painters use when they wish to represent the difference between a near and a remote object. We judge an object to be more distant from us by the diminution of its apparent magnitude, that is, by linear perspective; or by that dimness or faintness of colour and outline which generally increases with the distance, in other words, by aerial perspective."† Mr. Mansel's statement agrees with this, except that he allows that the convergence of the optic axes may perhaps aid us in estimating the distance of near objects. We shall see hereafter that the signs enumerated by Mill may be eliminated or reversed, one and all, without the judgment of distance being rendered one whit less decided and irresistible. In this respect, he has not improved upon Berkeley, who enumerates as the signs of distance—1st, the convergence of the axes; 2nd, confusedness or faintness; 3rd, straining of the eye, besides other secondary signs. The principal difference between him and his successors is, that he held (sec. 158) that planes are no more the immediate objects of sight than solids; whereas the modern doctrine is, that the immediate object of sight is a plane variously coloured. In a word, the theory has been exactly stated by Swift, "Vision is the art of seeing things invisible."

I shall endeavour to show that this theory proceeds on assumptions which can be proved to be false; that it supposes processes which are opposed to all analogy and must be pronounced impossible, and that its results are in contradiction to experience. It is necessary however to make one or two preliminary observations. First, that the only account we can give at best of any perception is to show the mode in which the appropriate affection of the organ is produced. This, in the case of taste, for example, is by the solution of

* Dissertations, vol. ii., p. 89. † Ibid, p. 87.

the sapid matters in the secretions of the organ. With regard to most of the senses indeed we have not yet advanced even so far. We do not know, for instance, the exact effect produced by sound on the organ of hearing. But in no case can philosophy be called on to show how that affection can issue in such and such a perception. The opinion that a resemblance must exist between the perception and its antecedent will scarcely be explicitly maintained by any philosopher of the present day. Secondly, we must observe that the question is not about the origin of the idea of space, but about its empirical apprehension; nor is it the question, whether the eye can measure distance up to the limit of distinct vision, but whether it is capable of giving us the fact of distance at all, without the help of any other sense. As this distinction between the perception of distance absolutely and that of degrees of distance is very important, and as they have been explicitly confounded by Mr. Mill, I shall develop it a little more fully.

It is conceivable that sight might convey the idea of objects being at a distance, and even of one object being farther than another, yet without our having the least notion of the measure of that distance. For, consider the case of colour, which is admitted to be the proper object of vision, and which in fact, as colour, is simply a mode of sensation. Well then, let us present to a person not colour-blind several shades of green together: he will readily distinguish and arrange them in order of depth, lightness, &c.; but to recall them to mind and reproduce them is another matter. Ask the same person to define the depth of a particular shade seen apart from others, or which comes to the same thing, to say how much lighter or darker it is than one he saw yesterday, and in nine cases out of ten he will judge wrongly. Few persons, except those who are obliged to attend to these sensations, are able to reproduce them with any degree of

accuracy. In illustration of this, it may be mentioned, that in the discussion between Mr. Airy and Sir David Brewster about the solar spectrum some years ago, the former, himself a practised observer, asserted that "the eye has no memory for colour." Brewster, in his reply, stated that his own memory of colour was perfect.* If the question be of the pitch of a sound, we shall find still fewer persons able to give an accurate judgment. Yet it would be absurd to say that every man who cannot accurately designate shades of colour is colour-blind; or that unless a man can tell what note or chord he hears when a piano is struck, he is insensible to music, and all tones sound alike to him. If he can distinguish colours or tones when in juxtaposition the defect is not in the sense. It is the same with perceptions, as from the nature of the case it ought to be. We perceive resistance, but who can accurately measure the amount? We perceive muscular effort, and hence weight; yet how few can estimate either effort or weight, except in the roughest possible way.

The explanation is plain. To know what colour we see, what tones we hear, what effort we exert, means this, and no more:—to compare the colour, tone, effort, or weight with other sensations or perceptions of the same kind, and assign them their proper place in the scale. To speak of perceiving what an effort or tone is absolutely, is as unmeaning as to speak of the absolute place of an object. To know, then, what we see, hear, or feel, is not a matter of direct perception, but requires comparison and judgment. But as the other terms of the comparison are not simultaneously perceived, there is required further an effort of memory. But neither the power of reproduction nor that of judgment is proportional to that of perception, or in any way dependent on it. Instead, therefore, of the one operation of perception, we have three or five distinct processes; and the power

* Philosophical Magazine, vol. xxx., pp. 74, 153.

of perception may be conceived to exist in the absence of all the rest, as is very probably the case with some animals.

Having thus relieved the question of irrelevant matter, let us consider what is required in order to prove that the visual perception of distance is the result of association with perceptions of touch.

First, it must be proved that sight actually does not perceive distance. This may be shown *a priori* or *a posteriori*. The only *a priori* proof possible is a physical one—one, namely, which should show that the immediate object in perception must be identical whatever the variation in the circumstances supposed to be perceived. An objection founded on the fact that the variations are such as contain no necessary connexion with the idea of distance, is perfectly worthless, since the process of every sense is equally beyond our ken: nowhere is there any necessary connexion between the idea and its antecedent. *A posteriori*, it might be shown that persons deprived of sight are capable of the perceptions in question; while those who possess sight and not the locomotive faculty, are not. Secondly, it must be proved that distance is perceived by touch, or the locomotive faculty. This may be done by the observation just mentioned; or we may have recourse to the *Instantiæ variantes in proximo*, and show that the accuracy or power of perception of distance is proportional to the energy and exertion of the locomotive sense. Thirdly, the fact of the association between the perceptions of touch and the sensations of sight must be established; and fourthly, it must be shown, that the variations in the suggesting sensations correspond with those in the distance perceived. In stating these conditions, I have not transgressed the limits which philosophy and logic alike pronounce indispensable.

How have these conditions been satisfied? Berkeley has endeavoured to prove the first in the *a priori* method;

the second he takes for granted; the remaining two he does not seem to have thought of stating, much less of proving. Yet his work has been called "demonstrative." His deficiency, however, on the second head, others have endeavoured to supply, with what success we shall have to consider presently. Berkeley's argument is in substance the following:—It is physically impossible that the eye should be the organ of perceiving distance; but it is a fact that by sight we do judge of distance. Consequently, this must be by the suggestion of some other idea. Now, it is admitted that distance is perceived by touch, and if not by sight, by touch only. This, Berkeley (with Mill) apparently considers too obvious to require to be stated. Hence follows his conclusion, that the supposed perception of distance is a suggestion through visual signs of a tactual idea.

It is my purpose to show—1st, that the immediate perception of distance by sight is in no way impossible or anomalous; 2nd, that on the contrary, the assumed association of visual and tactual sensations does not exist, and is even impossible; and 3rd, that neither touch nor the locomotive faculty or sense is perceptive of distance. I propose to show further, that the eye is perceptive of distance—1st, by appeal to consciousness; justified 2nd, by the analogy of the senses and by physical considerations; 3rd, by the analogy of the lower animals; 4th, by special cases of congenital blindness cured in mature years.

First then, as to the physical difficulty, it is conceded, according to Berkeley,* that distance cannot be in itself perceived, being a line directed endwise to the eye. This has been generally regarded as unanswerable. It is in fact the sole positive argument advanced either by Berkeley or Mill. The latter puts it in these words, in a very clear and convincing manner:—"The distances of objects from us are re-

* Essay, sec. 2. Three Dialogues, Dial. 1.

presented on our retina in all cases by single points; and all points being equal, all such distances must appear equal, or rather we are unable to see them in the character of distance at all." "As the interval between an object and our eye has not any interval answering to it in the retina, we do not see it."*

Now, instead of regarding this argument as decisive, we ought to set it aside at once, as unsound in principle. It is absurd to impose *a priori* conditions on the perceptions of sense or other physical phenomena. Indeed, in all other departments of science it has been for centuries recognised that our business is to ascertain, not what the phenomena ought to be, but what they are; not to deduce effects from their supposed causes, but to inquire, in the first instance, what are the effects.

If, however, we chose to indulge *a priori* speculation, we might demonstrate, in imitation of Berkeley, that the only qualities of sound which can be heard are pitch and loudness. To speak of tone, timbre, &c., of distinguishing strings from pipes by the ear is absurd; since the vibrations of the tympanum can differ only in extent and velocity; and, therefore, all sounds which are alike in these respects, *i.e.*, in loudness and pitch, must produce identical sensations. Yet the fact is, that we can distinguish the tones produced by strings, pipes, reeds, by metal, wood, the human voice, &c., and even in each class can trace an almost endless variety of character. We can recognise a friend by his voice as well as by his face. A Berkeleian ought, by analogy, to maintain, that in these cases we do not hear directly the tone of our friend's voice or of his violin, but that the visual ideas of him and of his instrument are suggested to us, and falsely referred to the ear. And, no doubt, this would have been affirmed if it had been possible to discover any intermediate ideas to which

* Dissertations, vol. ii., pp. 95, 116.

the suggestion could be attributed. As to the particular argument now before us, it may be applied to prove that distance cannot be directly perceived by any sense. If both ends of the line affect the same spot of the eye, touch labours under the more fatal defect of not perceiving the farther end at all. Locomotion does not even pretend to be a means of direct perception of an interval. By its means, indeed, we may be cognisant of certain points in the distance, but not as points in the same right line, and not simultaneously; and as for distances within the reach of our arms, it may be said, as of the eye, that since different distances from the organ will produce the same effect, there can be no perception of distance at all. There is no affection of the organ answering to distance. If we grasp an object we do not perceive at what distance our hand is from our body. The fact is, that the argument implies that the antecedent to the perception of distance must be a sensible interval in the organ, in other words, that the organic affection must resemble the perception. But, as just remarked, we can assume no such *a priori* conditions; and such a condition as this would exclude every other sense as well as sight.

These considerations would be enough to set aside the objection if the statement on which it is founded were true; but it is false. It might be questioned even as regards a single eye; but the organ of vision consists of a pair of eyes, a fact of which Mr. Mill does not betray the least suspicion; and distance accordingly has an interval corresponding to it on the organ—an interval, too, of a very peculiar kind, which cannot be confounded with that belonging to lateral distance. As, however, the further consideration of the physical question must be resumed hereafter, I shall now pass to the examination of the supposed association.

CHAPTER II.

ON THE LAWS OF ASSOCIATION.

It is admitted that we cannot by any effort of attention recover or bring into consciousness the steps of the suggestion supposed. This at once separates it from all those phenomena which are alleged as analogous, as well as from all other proved associations. There is no stronger association than that between the ideas of common things and their names; so that uneducated people find it hard to understand that it is only accidental. Yet, with very little pains, this association can be resolved, and even changed; the very conception of a language different from our own implying such a resolution. Again, the association between written characters and the sounds they represent is, to persons who read or write much, one of the strongest. Yet here again there is no difficulty in distinguishing the association from natural connexion, and but little in establishing a different one. If it is true, again, that words, "do their work as signs . . . with the faintest possible suggestion of most of the sensible ideas which compose their meaning,"* it is also true, that we can, if we will, arrest this process, and fix our attention on any part of the sensitive perception, and can thus separate the part of sense from that of intelligence.

Since, then, it is not pretended that this analysis of the phenomena of visual perception can be "demonstrated" by observation, we are compelled to examine the principles of association in order to discover whether it is in accordance with them.

* Mill, l. c., p. 103.

The fundamental laws of Association have been reduced to two—the law of Redintegration and that of Repetition. By the former, with which alone we are concerned, those thoughts which have at any time formed part of the same mental whole, have a tendency to suggest each other. Hence, first, they must have co-existed—they must have existed simultaneously—if a series of thoughts have intervened, it will effectually bar all *direct* association; and, secondly, the tendency to suggest is mutual. If the thought A recalls B, then B also tends to recall A. This law is modified by certain secondary laws. If A have been associated with many other thoughts, and B with few besides A, then B will be more likely to suggest A than *vice versâ*. For example, in reading, the sight of the words suggests their sound and signification, while the converse is not the case with any but constant readers. It is for the same reason that a man who does not readily recollect the names of persons he meets, yet may find no difficulty in remembering the person when the name is mentioned. So also with most persons the words of a song are not so readily suggested by the air as the air by the words.

Secondly, the association will be strong in proportion to the frequency of co-existence of A and B. Unless the attention has been peculiarly excited a single instance will not generally effect their connexion; but after several the one may even invariably call up the other. It will of course be remarked, that whenever A occurs without B, the association is practically weakened, inasmuch as new thoughts will struggle with B into notice on the next occasion. To state the law generally, the tendency of A to suggest B is greater in proportion to the frequency of their connexion, and less in proportion to the frequency of A's connexion with other thoughts. Strictly speaking, B is suggested equally, whether A have other associations or not, but in the latter case it com-

mands undivided attention, whereas if other thoughts are present, they tend to suppress it.

Thirdly, the strength of the association is proportioned to the original vividness of the combination. More exactly, if the vividness of A and B, at the moment of their association, were α and β respectively, then A tends to raise B to the vividness β, and with a force proportional to α, or to its present vividness, if less. In proportion to this force is the velocity with which B is revived.* Thus, if we try to recollect the place where we have put away a certain key, its suggestion will be rapid in proportion to the attention given to the key at the time of so placing it. If, again, we are thinking of some general principle, and desire to recall instances or illustrations of it, those will occur most rapidly which we have already connected with it; those more slowly which have been referred to kindred principles; and, last of all, those in which no such relation was hitherto remarked. In the case of a succession of thoughts or perceptions, as each new link commands attention, its predecessor becomes indistinct. This is peculiarly the case with perceptions, which are reduced to representations when the object is removed. It is obvious that if B is weakly suggested, it may be wholly suppressed by other thoughts which are revived at the same time but more rapidly. In this way the certainty of revival is in some measure dependent on its quickness. It is an example of this law, that when we have striven in vain to recollect some name or phrase, it will often occur as we turn over the leaves of the book in which we found it. It appears to be from the same principle that reporting a speech verbatim interferes with the remembrance of it. The more the

* This law is due, so far as I know, to Herbart (Psychol. als Wissenschaft, sec. 86). The statement in the text is sufficiently accurate for our present purpose.

attention is monopolized by the present word, the less powerful is its association with the preceding.

All thoughts of which we are not actually conscious, but which are capable of being revived by suggestion, may be called *latent*. Now, it is important to remark that there are degrees of latency; some thoughts have sunk deeper into unconsciousness than others, and require a greater force to revive them.* Thus, what has been long forgotten is, *ceteris paribus*, harder to recall. It has, so to speak, sunk deeper into unconsciousness. Now, this being understood, the vividness of a reproduced thought is proportioned to its greatest previous vividness.† This is the limit which, by the law of Repetition, it tends of its own force to regain; but which

* See again Herbart, l. c., sec. 47.

† This law, combined with the preceding observation, contains, I think, the explanation of those facts from which Sir W. Hamilton inferred what he calls the Law of Preference, namely—"Thoughts are suggested, not merely by force of the general subjective relation subsisting between themselves. They are also suggested in proportion to the relation of interest (from whatever source) in which these stand to the individual mind." (Dissertat. on Reid, p. 913*a*). He adds, most marvellously, that this relation "determines the suggestion of a movement not warranted by any dependence on its antecedent." This is very like saying that an effect precedes one of its conditions. The interest of a thought is its power to excite our feelings and attention; but this cannot be manifested until the thought has come into consciousness, and therefore cannot help to revive it. But if a thought be interesting, it has probably excited a proportionate amount of attention or emotion on a previous occasion; probably also it has appeared both frequently and recently. For the latter reason it is less latent, and therefore more prompt to recur; and for the former reason, it is more vivid when revived. Further, if suggested by a thought which has few associations, it will be less impeded. It is easy to apply these principles to the example given by Sir W. Hamilton. "The sight of Tobias' dog calls up the image of Tobias in the mind of his mother with a far greater vehemence, than does the sight of Tobias call up in her mind the image of the dog." When we make sufficient allowance for the influence of these causes, there will, I think, be nothing left to justify the assumption of an additional and occult principle of preference.

could be fully attained only in the absence of all opposing thoughts. Owing chiefly to the resistance of these, revived thoughts are in general less vivid than at first. One case however requires special notice. A perception reproduced is no longer perception but representation. The distinction is unmistakable, and is recognised by philosophers of every school. It is sufficient to quote the words of Hume, to whose philosophy it was a stumbling-block :—"All the colours of poetry," says the great sceptic, "however splendid, can never paint natural objects in such a manner as to make the description be taken for a real landscape. The most lively thought is still inferior to the dullest sensation." This superiority of perceptions in "vivacity and force" appeared to him to justify their elevation into a distinct class by the name of "impressions," of which "ideas" were only the copies. A more important distinction, however, is that the immediate object of thought in representation is not believed to be external to the mind itself. This leads to the most important consideration for our purpose. In perception we have an immediate knowledge or apprehension of an object external to our mind. Perception is in fact this knowledge or apprehension. So far from being known merely as a state of mind from or in which we judge of the existence of external causes, it is not known as a mental operation except by a careful process of reflection. To the common man perception is the direct knowledge of the external; but the same man never mistakes a representation for a perception.

Fifthly, according to a law pointed out by Sir W. Hamilton,* this representation of a perception takes place in the appropriate organ of the original perception. The face we have seen we see again with our "mind's eye." The sweet song

* It was not, of course, original to him, since the same principle had been adopted by the phrenologists.

that we have listened to with delight, is still singing itself over in our ears—

> "Music, when soft voices die,
> Vibrates in the memory;
> Odours, when sweet violets sicken,
> Live *within the sense they quicken.*"*

This is indeed a case of the more general law, of great importance, although hardly noticed by psychologists, that the representation of any mental phenomenon takes place through the faculty appropriate to the original presentation.†

* Shelley.

† See North British Review, vol. 30, p. 560.

CHAPTER III.

APPLICATION OF THESE CRITERIA.

If now we examine Berkeley's theory by the light of these principles, we shall find that it violates them all.

First—Is it true that the sensations of sight are constantly associated with the perceptions of touch? This has been universally assumed, with apparently no better foundation than the fact that we are constantly receiving sensations of sight, and are also constantly perceiving by means of touch. Take an instance. We open our eyes, we look forth on a multitude of objects at varying distances in our room. None of these do we touch, except at this moment the pen and the paper or the book. We touch also the footstool and the chair at several points without seeing them. So it is at all times. Countless objects are presented to the eye, and of these very few to the touch. Consider then those that are objects of both senses—the pen, for instance. True, I see a coloured outline, either not without the mind at all (Berkeley, &c.), or, at most, not distant from the organ. I feel also something pressing on four distinct points of my fingers and thumb; but of its shape, and whether it is one object or many, these give me no information: they tell me only that at these four points I touch something smooth and hard. Here then are two sets of ideas constantly conjoined: the vision of a certain form in the eye, and the impression of a certain hardness and smoothness to the touch. We shall waive the question of shape. Now, every object in the room is seen a thousand times without being touched, for once that it is the object of both senses. Look out on a landscape, and the observation

holds with immensely increased force. What countless objects we see, at distances no less various, not one in ten thousand of which we ever touch, still less think of measuring its distance; yet it is a fact that the pen and every one of these countless objects appear to be seen at a certain distance, whether we intentionally dwell on the idea of distance or not. But how then has this association taken place? Berkeley has not attempted to give any explanation: he simply takes it for granted. Mr. Mill does the same. Mr. Bain, however, enters more particularly into the question, and resolves the visual perception of distance into the association of ideas of locomotion. My idea of distance, he considers, is merely the idea of a certain muscular exertion, necessary to produce contact. We shall look into this more closely by-and-by; at present let us simply ask, how is the idea of this muscular exertion associated with those of colour, ocular adjustment, &c.? On the one hand, I use considerable exertion in moving the pen to-and-fro, without much altering its distance. On the other hand, I do certainly now and then unconsciously bring my hand to my head or my knee; but then the part of the pen to which my eyes are directed and that which my fingers have touched are different, and judged to be at different distances. And this is the ordinary case. It is only in an infinitesimally small number of cases that the eye and hand bring reports simultaneously from the same point. For it must be remembered, that the association must be established separately for every distinguishable adjustment of the eyes, and for every distinguishable amount of muscular exertion, as well as every degree of brightness and confusion. Having associated the sensation corresponding to a certain angle and convexity with a certain distance, we have not made the slightest step towards connecting half the angle with double the distance. Is it answered, that it is done by a natural geometry? Then we fall into the absurdity justly exposed

by Berkeley. Or is an instinct postulated for this calculation? Then all this laborious analysis has been undertaken in order to deny for the first step an instinct which we are obliged to assume for the second. It will hardly be affirmed, that the fact that all the intermediate angles must be passed through when the inclination of the axes changes, even from 0° to 10°, furnishes the means of connecting different series of associations. It is necessary then that for every one of these innumerable modes of sensation an independent association should be established through constantly repeated combination with its correspondent muscular sensation. We see a house at the distance of forty yards, a mountain at ten miles. Such objects are no doubt constantly presented to our eyes; but how often do we estimate the distance by any other sense? Not once in ten thousand times. Where is there the remotest approach to the constant association which the theory requires? The supposition that there is any such is so monstrously false, that it is difficult to render its absurdity more clear by any explanation. This being so, we ought to be able all the more readily to show the growth of the association. We might expect the steps by which the eye acquires a faculty strange to it to be pointed out, not in theory, but in experience. That this has not been attempted I need not remind the reader; that it cannot be accomplished—that experience unequivocally contradicts it—remains to be shown.

But I proceed to the next condition, the reciprocal suggestion of associated ideas. It has been observed, that when two ideas are associated each tends to suggest the other, not only in proportion to the frequency of their association, but also to the exclusiveness of it, *i.e.*, to the degree in which the first has been free from other associations. Let us then consider in the case before us which of the two ideas has been most free from other associations. To state the ques-

tion is indeed to suggest the answer. It has been already noticed, that for one object we touch we see a thousand—a thousand thousand would be nearer (and yet far below) the mark, when we remember that we see every point of a vast complex picture at distances infinitely various, while only a few points can be the objects of other senses. Hence it is clear that the visual ideas must be associated with a thousandfold—nay, infinitely more—ideas than those of touch or locomotion. Consequently, if there be an association, it is the latter which ought to suggest the former. But what is the fact? Do we find the tactual or locomotive perceptions suggesting those of sight with the same constancy that the converse is supposed to occur? Does the consciousness of muscular exertion recall the corresponding visual sensations? For instance, when we put out our arm to touch a known object in the dark, does the effort suggest either the degree of brightness and distinctness or the apparent magnitude corresponding to the distance or the appropriate adjustment of the eye? Or again, when we walk a few steps in the dark, does the labour revive in our imagination the visual sensations corresponding to the interval traversed? There is not the slightest tendency to such a suggestion; on the contrary, the endeavour to recall these sensations is attended with difficulty and uncertainty. The idea suggested in both cases is independent on the exertion used. Contrast now with this the phenomena of the unquestioned association between the colour and the hardness and roughness, &c., of granite or other stone. A certain hardness, roughness, &c., suggest at once the colour of stone, and conversely the colour suggests the other qualities. Nevertheless, we do not suppose when we handle a piece of granite that we feel the variegated colour or any other constantly associated object of sight. The second condition of suggestion is therefore opposed to the hypothesis.

But again, waiving these arguments, and granting the association and suggestion supposed, we need not be reminded that amongst the ideas of touch those will most readily occur which have been most frequently connected with those of sight. But which are they? Certainly not distance. We rarely measure the distance of the objects we touch. There are ideas, however, which never fail to be presented when an object is touched, namely, those of hardness and resistance in their degrees. These are associated with a certain visible appearance—a certain position of the object, and, I may add, with the touch of the surface. Do we then ever believe that we see hardness or resistance? Berkeley, indeed, thinks the difference between this case and that of distance is only one of degree. If it were so, the advantage ought to be on the side of the ideas most frequently presented. But the difference is one of kind; for the phenomenon to be accounted for is, that we believe the extension or distance to be an object of sight; whereas we never believe that we see hardness; we only say an object looks hard, or otherwise—that is, has the appearance usual in hard objects.

The association has been shown to be insufficient and incompetent to produce the effect ascribed to it. I shall now proceed a step further, and prove that it is impossible. Granting that on every occasion on which we have seen an object we have ascertained its distance—and not only that, but what is equally necessary, the distance of all its parts—by touch or otherwise, let us see whether that is sufficient to establish an association. No advocate of Berkeley will be bold enough to assert that touch gives us an immediate perception of distance. In order to ascertain by any modification of the sense of touch the distance of an object beyond the reach of our arms we must move towards it. By the time we have arrived at it we may be presumed to have an

idea of its distance independent, at least, on sight. But now that we have, perhaps with great difficulty, made sure of this perception, where is the visual sensation which it behoved to be associated with it? Numbered with the things that were. Even with the utmost attention, it is scarce possible to retain for a moment the idea of a particular degree of distinctness, and still less of ocular adjustment. But if we have not painfully watched it, and with closed eyes guarded its purity, it cannot escape extinction, or still worse, confusion, in the constant succession of similar and gradually changing impressions. When at last we reach the object, the idea or sensation which is really present is not the visual idea which we had when we started, but one which we do not at all want to associate with our carefully acquired perception. The processes of nature, however, do not wait for our wishes; an association will take place, but with the latest appearance of the object—not with its first. The association required, therefore, cannot take place, for the simple reason that the ideas to be associated cannot co-exist. We cannot at one and the same moment be looking at an object five, ten, fifty yards off, and be achieving our last step towards it.

Perhaps it may be objected that we do actually estimate distance in some such way as that supposed. By frequently measuring distances we acquire a facility of judging by the eye, which is best accounted for by association. But what is the process? We first roughly estimate the distance by the eye—this we correct by measurement. But the corrected judgment is not immediately associated with the visual ideas belonging to that distance; on the contrary, the next time we attempt to estimate a similar distance we call to mind that we made in the former instance an error of, say fifty yards in excess, and that, therefore, we ought now to correct our first estimate in proportion. But this supposes that

prior to correction we should make some judgment. Let us suppose, in fact, that there has been no judgment. We may proceed by memory and reasoning—the present appearance recalling the former, we may recollect the measured distance. But this implies a conscious measurement in the first instance, and that we have laid up the result and the visual sensations in our memories. If there has been no intention of measuring we can only affirm that a certain visual appearance has been followed by a train of ideas, amongst which are a series from touch; but these are the least vivid, and usually pass without notice. Attention is not, indeed, necessary to association; but if, as in the present case, the perceptions are separated in time, then attention is necessary to retain the ideas, which otherwise would be lost before their intended fellows appeared. This is confirmed by an experiment on a large scale. "Judging distance" is now the subject of daily instruction to thousands in England; and the preliminary guessing is found to be indispensable. But according to the received theory, the most certain and philosophical method of learning to estimate distance would be by simply marching, and observing the sensations, visual and muscular. This is decidedly contradicted by experience.

All this applies only to comparatively short intervals—those which we can and do measure frequently, though not consciously. But when we come to consider greater distances the theory breaks down, if possible, still more irrecoverably. How do we in practice estimate larger distances? By comparison and multiplication. We judge one interval to be double another, and so on. But there can be no multiplication of sensations. Any two are wholly distinct, and cannot have a numerical ratio. But when we see a distant mountain where are the perceptions supposed to be associated with those of sight? If any perception of near distance is given by sight we can extend

and apply it to the estimation of greater distances, but not otherwise.

Equally decisive is our third law, that the force of suggestion is in proportion to the vividness of the suggesting thought in the original combination. Here the two things connected are tactual perceptions and visual sensations. Now, the sensibility to minute differences in brightness is so feeble that most persons do not even recognise them at once when their attention is expressly directed to the subject; and who will affirm that in infancy we were more sensible to these delicate shades of difference? On the other hand, the theory itself supposes that the tactual perceptions absorbed at first a great deal of attention. We should expect therefore that the suggestion of these perceptions by the sensations would be much slower than the converse suggestion. The case is indeed very analogous to that of the recognition of musical intervals by persons of moderate knowledge of music. We have a vivid conception of the written intervals, thirds, fifths, &c., but an obscure one of the difference to the ear of the same intervals. Accordingly, in trying to read music, we find that the written intervals suggest the audible with corresponding rapidity, but at the same time obscurely and inaccurately; while on the other hand, if we try to write down an air from ear, it is but slowly and painfully that we think of the intervals to be written; but when they do occur, they are almost as clear as if they were actually before us. I do not speak, of course, of their accuracy. Now our conceptions of differences of brightness, &c., are as obscure as those of musical intervals, while the perception of differences of distance is as clear as that of the written notes. Consequently, by the same laws, the difference of distance ought to suggest the difference of brightness and distinctness with more quickness although with less vividness than the converse. Now, what is the fact, if the theory be admitted? Why, that the

difference of distance does not suggest that of brightness at all, while the converse suggestion is not approached in force or rapidity by any other known. What has been said of brightness and distinctness applies with even greater force to the muscular movements of the eye, of which we are in fact wholly unconscious.

So far it has been shown, 1° that the supposed association cannot take place; 2° that if there were any such we should see not distance but hardness and resistance; 3° that the suggestion would be mutual, and that the ideas of touch would be more likely to suggest those of sight than *vice versâ.*

Now let us apply the condition we have stated, that a perception is reproduced in the appropriate organ. A written melody at once suggests to a musician the musical effect; but he never imagines that he sees the song. Nor do we fancy that we smell the purple colour of a violet which we do not see. We see the lips moving, and we think we hear words which have only imperfectly reached our ears, or not at all; but we never think we see the sound. In the same way, if a perception of touch be revived, it must be as the representation of a perception of touch, not as a perception of another sense. There would be no need to dwell on this point if the current doctrine had not obscured it. In no other case has the slightest approximation to such a transference been pointed out. Bishop Berkeley, indeed, with more acuteness than most of his followers, admits the difficulty,* but affirms that notwithstanding the common prejudice, we no more see distance than we hear the hardness, resistance, colour, and figure of a rock, when we hear the name. This reduces the question to a simple one, to be answered by an appeal to consciousness. To take Berkeley's instance, the association between the word "rock" and the idea of hardness is as

* *e.g.* "Minute Philosopher," Dial. iv. sec. 11.

firmly established as any other can be. Hearing the word, we see in our minds a certain form and colour, we feel immovability and hardness. Do we suppose that we *hear* any of these? Not in the least. At the thousandth time of suggestion we are as far from this as at the first; nor is there the least prospect that any amount of association would effect such an illusion. On the other hand, it is the clear, unmistakable deliverance of consciousness that we do see distance, and that we do not imagine ourselves feeling it in any way whatever. Whatever suggestion may have to do with it, the idea is not reproduced as an idea of touch or of locomotion. When we look up to the stars, we see them to be at a distance—we do not *feel* the distance, or imagine the exertion necessary to walk it. In this the philosopher and the uneducated are at one. To quote only Mr. Mill—"The things which it (the eyesight) discovers by its acquired powers seem to be perceived as directly as what it sees by its original capacities as a sense. Though the distance of an object from us is really a matter of judgment and inference, we cannot help fancying that we see it directly with our eyes."*

And how does analysis alter our judgment? No acuteness of analysis enables us to separate the visual perception from the visual sensation. We cannot see things as in a plane in contact with our organism. "No man," says Stewart, "by any act of his can cease to see distance." Nay, more, when we know that the surface is a plane, as in a mirror, we cannot see it as such. On the other hand, we never see resistance or hardness. Yet it has been shown that resistance is much oftener associated with ideas of sight than the tactual measure of distance. If then no one is tempted to believe resistance an object of sight, we have an association much stronger than that which is supposed in the case of distance,

* "Dissertations," vol. ii. p. 88.

yet without the least tendency to produce a similar effect, The evidence appears to me decisive, and the criterion destructive.

But we have not yet done with this criterion. There is another case of reproduction to which it applies with, if possible, greater force. We have hitherto applied the method of Agreement; let us now apply the method of Difference. It constantly happens that ideas of distance, extension, &c., are suggested to the mind, without any visual sensation having preceded, either by words or by other conceptions, or in the dark by touch itself or by locomotion. Now, I ask the reader to consider with himself how these ideas are reproduced in the mind, in what organ of sense? I ask for a careful examination of this question, for here lies the *experimentum crucis.* Is then distance suggested as an object of sight, or of touch, or of the locomotive faculty? If there could be any doubt, we might spend some time in analyzing the recollection, but probably there will be no difference of testimony. All will doubtless agree what we imagine is the sight of the distance, not any feeling or effort; but, according to the law under consideration, if distance be recollected as an object of vision, it must have been originally so. We may go further still. Let us ask, what is the mental process when we *feel* an interval with our fingers, the eyes being shut, and the sight of distance suggested as it usually is. On Berkeley s theory, and on Bain's, the touch first gives us the perception of the length which, itself unnoticed, then suggests the visual sensations, and these in their turn suggest the original idea of touch; the actual perception thus giving way in an unheard-of manner to a mere representation, and that only a secondary suggestion of a similar perception. Let the reader try by careful attention to recover the steps of the process, and then judge whether this be a true account. If not, if it be rather an utter absurdity, then it appears that so

far from touch enriching sight, it is actually obliged to borrow from it.

This leads to another condition, which remains to be considered—the inferiority of a representation to the original perception in clearness and perfection. This surely needs no proof. The loss of accuracy, however, in remembered perceptions is not merely matter of conjecture; it admits of actual measurement. This has been to a certain extent accomplished by the experiments of E. H. Weber. First, as to the power of estimating weight. Most persons can distinguish weights which are in the proportion of 39:40, when handled in immediate succession. But if from fifteen to thirty seconds be allowed to intervene between the first and the second impression, the proportion must be not closer than that of 29 or 28:30. If the weights are as 4:5, they can be distinguished at an interval of from 60″ to 90″. Thus after one minute and a-half the estimation has only one-eighth of its original accuracy. By sight, again, we can distinguish two lengths which are as 10:11, if observed at an interval of not more than 70″; if the lines are as 20:21 they cannot be distinguished unless observed within 40″; if as 50:51¼, not more than 5″ or 10″ must be allowed to elapse.* In this case the representation loses its accuracy at about the same rate as in the former. How then, it may be asked, can we ever compare distant observations, as in practice we often do? Simply by the use of names of measure. By means of these we register our observations mentally, and are enabled to compare distant observations through the medium of a common and permanent standard. But in the case now under consideration, there can in general be no question of such mental registering under names, and there is therefore nothing to counteract this law of diminishing accuracy. On the other hand, it needs no proof that we estimate distance and magnitude by the eye

* E. H. Weber, in Wagner's Handw. Art. "Tastsinn," pp. 545, 547.

with more rapidity, accuracy, and certainty, than by touch or locomotion. Take as an instance the capacity of estimating short distances traversed in the dark, which is very different in different persons. There are many persons—I am acquainted with some—who will make an error of a yard in four or five in comparing distances so walked with the familiar visual judgment of them; who nevertheless are remarkable for accuracy in judging of distance and magnitude by the eye. Whenever angles have to be measured as well as intervals the difference is still more striking; yet, according to the theory before us, this latter judgment is a reproduction or copy of the former. Again, contrast the tedious and laborious process by which we gradually obtain by touch an idea of the figure of any body, from a flower to a mountain, with the almost instantaneous judgment of the eye.* Yet the latter will be infinitely more exact. If we go into a strange room of unusual shape in the dark, the utmost pains will not enable us to gain nearly as good a notion of its plan as the eye would give in an instant. Indeed, even in this simple case, Sir W. Hamilton observes that touch would afford us but slender aid.† We might handle a plant for hours without obtaining anything like a correct idea of its form. Let us open our eyes, and little more than a glance will give us the most perfect idea of which we are capable. And we are told that the process in the last case is that the visual sensations suggest the tactual perceptions, which have thus acquired a vividness and perfection unknown before, and that, be it observed, by losing the vividness and perfection which give perception the superiority over representation. It is true, if we are in doubt about points of detail, we apply the test of

* "Our knowledge of form is a very complex process, requiring not merely the exercise of the sense of touch, but also great attention to the muscular sensations."—CARPENTER, "Human Physiology," p. 688 (on Touch).

† Reid's Works, p. 132.

touch—but only for details. And when we try by touch to form the idea of the object as a whole, we do it by imagining it as an object of sight.

But we need not take the instance of an object so complex as a plant, vastly less complex though it be than other objects which can be taken in at once by the eye. Take a simple body, one which we can touch all at once—an egg, for example. We feel it, turn it over and over, and with some pains obtain a vague idea of its form. Now, if we open our eyes, and withdraw the hand, we have no longer the direct perception of its magnitude, figure, &c.; but these ideas are suggested to us by visual sensations, *i.e.*, we have now represented in imagination the *same* idea which we have just obtained by handling. The same idea! Why, the contrast is as great as between the light of the sun and that of the moon. But now it is the reproduced image which eclipses the original—the satellite whose brightness overpowers that of its central light! Is there a parallel instance in all philosophy? Nay, "nought but itself can be its parallel."

The case is, however, much stronger than I have stated it. For first, be it observed, that in looking at the supposed egg we have not had the advantage of handling that identical object before; we have not compared the ideas it furnishes to sight and touch respectively. The figure suggested by sight, therefore, is *ex hypothesi* not given, except in its elements. In order to have the idea of a continuous figure like an oval or ellipsoid, we must perceive a vast series of regularly varying intervals. For every *minimum visibile* included in the object there must be suggested a certain distance from the eyes as well as a certain distance from the bounding outline. All these must be suggested with such accuracy that they may collectively correspond to the distances of the parts of an oval surface. This is the next step: to combine them, so as to get the idea of such a figure. And

observe, this may be done, although we have never handled anything similar. Supposing that to be the case, a process of suggestion and construction must take place, which we need not analyze, but which is obviously complicated in the highest degree.

Are we told that the operations of the mind are inconceivably rapid, and that all this seemingly tedious business may be instantaneous? Let us see to what this amounts. Rapid as the working of the mind may be, five steps must take more time than one, and the chances of error must be five times multiplied. Now, let us discard all the steps of this roundabout process which only take from the certainty and quickness. Let us handle the object with the eyes shut, and now we receive at once, and without the need of any secondary process of suggestion, reproduction, and representation, the very ideas which it took us so much trouble to attain before. But what ! how is this ? We cannot tell so very rapidly what the shape of the object is at all—we must feel it a little more. Ah! we begin to see it now—we can form some idea of it. What, SEE! Have we then after all come to this, that our tactual sensations are to suggest a visible image, in order that we may judge rightly of the figure of the object? It is even so. It is only by trying to imagine how the lines and angles would appear to the eye, that we can obtain a conception of the form of an object by handling it. And before proceeding further, I must direct attention to the illustration this affords of the supposed possibility of so complex a process as that spoken of being almost "instantaneous." In the case just put we have only a third of this process, and that the easiest step : we have namely the tactual sensations and perceptions given directly and together; and it only remains to combine them, in order to have the visual picture. How painful and tedious, and yet uncertain and imperfect, this process is every one knows. I have instanced in a very simple

case. But how vastly is the difficulty increased when we have to deal, say with an elaborate piece of carving, or still more a piece of nature's handiwork? Yet, to this difficult and uncertain process add another equally difficult and uncertain, and lo! a wonder unknown to science, the new complex operation is easy, certain, and delightful!

But at least if we could get over this primary difficulty, we might suppose that the process of suggestion on subsequent occasions would be facilitated by the handling we have supposed. The result of this must doubtless be to prevent the necessity of that doubly complex process of separate suggestion of the elementary components, and of their combination. The sight of the shaded figure will then at once recall the idea of the solid. Strange to say however the process does not become a whit easier or the result in the smallest degree more exact by the subtraction of difficulties. We see with the eye exactly what we saw before and no more, and exactly as we saw it. Thus the received theory here gives us a part greater than the whole, and a quantity increased by division. Where, again we may ask, is a parallel case? Take a single instance, the distinction of coins. Most persons find some difficulty in distinguishing by touch the size of a florin and a half-crown, or even of a sixpenny and a fourpenny piece. A single look however suffices. And we are asked to believe that in this case we distinguish the coins by means of the very same ideas represented, which we found it so difficult to distinguish before, when directly presented, and therefore more vivid and distinct. A blind man will distinguish better by touch, because his touch is practised; therefore clearly ours is not. Is not this a demonstration that what we distinguish are not representations of tactual perceptions?

The comparative delicacy of the two senses in the appreciation of distance may be directly estimated in the following simple manner. Let the reader take a number of peas or other

small objects, and first with his eyes closed arrange them in a straight line from him, in a second straight line perpendicular to this, and lastly in a circle, or ellipse, or square. He will find that in his first trials his errors will range from one to two-thirtieths at the least. Now let him make a similar arrangement with his eyes open, and he will find that (for small distances) the errors will be at the most about a third of the former. In fact at a distance of eight inches we can clearly discern a difference in depth of one line, as will hereafter appear.

For the sake of argument, I have only assumed that the process of combining a variety of suggested distances, so as to obtain the conception of the solid figure, is a comparatively complicated one. We need not hesitate however to pronounce it altogether impossible. If by frequent handling we have associated the visual and tactual perceptions of an egg, for example, then the sight of a similar figure will suggest the idea of the same form. But if the figure seen is one of which we have no experience, the suggested perceptions give us at best the distances of various parts of the surface from our eye. To deduce the solid figure from less distances would be equivalent to the solution of a complex algebraic equation. But the depth of the figure depends on the differences of these distances, and as the distances themselves thus given are obviously only approximations, the small differences may not be even like the truth. Now if, as Carpenter observes, it requires "great attention to the muscular sensations" to ascertain the form of an object by touch, when these intervals are given directly, and with the highest possible vividness and accuracy, it is not too much to say that in this way of suggestion it is absolutely impossible to obtain even a rough approximation to it.

CHAPTER IV.

ILLUSIONS OF SIGHT, AND CONDITIONS OF ITS PERCEPTIONS.

It may be objected that we have recourse to touch for information as to the minute structure of the surface; that the eye may be deceived by light and shade, &c., may mistake the plane for the projecting, and *vice versa,* and that touch alone can correct the error. It is true that touch gives more accurate information as to the smoothness or projections of a surface; but this circumstance need not present any difficulty, nor does it yield the slightest support to the suggestion theory. For in the first place, after touch has ascertained the form of the surface in any particular case for the hundredth time, still the eye does not adopt the discovery: it sees the object as it saw it before. Did it present a plane aspect before, it does so still; or did it seem convex, it does so still. A picture painted in good perspective suggests the idea of distance at least as vividly the thousandth time as the first; nor do objects seen in a mirror cease to appear at a distance behind it, even for the moment that we feel the solid wall before their apparent place.* In the second place, beyond this perception of smoothness touch does not go; it is itself equally at fault, if the deviation from planeness be gradual. It cannot distinguish a slightly ovate body from a sphere; and very slight inequalities escape its notice, which yet are readily perceived by the eye. Nor is touch more exempt from error even within its own domain. Within a

* On the necessity of a distinct association for such a case as this, see Berkeley, secs. 72, 73. He is obliged to assume that the same visual sensations suggest different ideas, according to the direction in which we look, whether vertical or horizontal.

certain limit, for example, two distinct impressions are perceived as one; this limit being fifty times as great on some parts of the body as on others, and admitting of considerable transient diminution by practice.* Again, if with the eyes closed and the hand resting on a cushion, we allow another person to touch the tips of our fingers with glass, paper, leather, &c., moved over them, we shall confound substances and forms which with the hand free to move, we should distinguish at once. Thus a plane plate of glass which is pressed first lightly, then more strongly, and again lightly on the finger-tip, will appear to be convex; if, on the contrary, it is pressed first strongly, then lightly, and again strongly, it will appear concave.† This is quite analogous to the illusion of the eye by light and shade. But is touch then to be pronounced incompetent to perceive the interval between two locally distinct impressions, or to distinguish the convex from the plane or concave? Or, again, must we deny the muscular sense the power of perceiving weight because this perception also is liable to be disturbed by accidental circumstances? For example, when we compare different substances, a very large body is judged lighter than a small one of the same weight. Weight and bulk being the same, an object which is easily grasped seems lighter than one which is more awkward. Even the same object may appear lighter or heavier, according to the extent of surface touched. Thus a truncated cone feels heavier when poised on the narrow than on the broad end. And all these circumstances being alike, a cold object feels heavier than one which is warmer.‡ And it has even been found that warmth is liable to be confounded

* See E. H. Weber, Art. Tastsinn, in Wagner's Handwörterb., p. 539. On the temporary effect of practice, see Volkmann, Berichte der Sächs. Gesellsch. zu Leipz., 1858, p. 38.

† E. H. Weber, l.c., p. 542.

‡ E. H. Weber, l.c., p 547, 551. ["De Pulsu Resorptione Auditu et Tactu," pp. 135–7.]

with pressure.* Sight itself is not affected by so many sources of delusion. Every faculty however has its limits; and touch and the muscular sense sometimes supply the deficiency of sight. But it does not follow that sunlight is inferior to a candle because we sometimes light a candle to search dark corners.

Indeed, as Graves observes—"The power we possess of correcting the errors of vision by means of the sense of touch, has been much exaggerated . . . . If we put the finger into water, it will appear to the eye to be bent at the surface of the fluid, *though we feel* that it is not so; and no matter how often such experiments are tried, the contradictions between these two senses will remain as strong as ever. In connexion with this subject," he adds, "I may observe that the sense of touch has been said to give more accurate ideas of shape than those derived through the medium of vision. A globe at a distance, it has been remarked, may appear to the eye flat, or a flat surface properly shaded may appear globular; and thus vision may induce us to form erroneous conclusions, which the sense of touch afterwards corrects. This mode of considering the subject is evidently unsound; for in the first place, we cannot by the touch alone decide as to the globular or flat shape of the surface in question, until we have felt it all over, until we have applied our fingers to it on every side; now if we take similar pains with regard to the evidence derived from vision, if we approach the surface, and regard it in various positions; in fact, if we look at it, not merely in front, but in profile, we shall be at no loss to distinguish that which is flat from that which is globular."† Generally, however, nothing more is needed for this than to view the object closer.

* Wunderli, "Beiträge zur Phys. des Tastsinns." Moleschott's Untersuch., vol. vii. p. 393.

† Graves, "Studies in Physiology and Medicine," p. 226.

For it must be observed that the cases of small and of great distances are quite distinct, not in degree, but in kind. The adjustments of the eye which enable us to perceive distance have of course their limit; the same interval becoming less sensible in proportion to its remoteness. Beyond a certain short distance, and for intervals too small to be directly perceived, we are guided by those other signs which have been often described. If the eye is liable to error in interpreting these, that throws no doubt whatever on the visual power of perceiving distance within the proper limits. No one doubts that we perceive lateral intervals by sight, although when very small or at a considerable distance they escape our notice. The variation in shade, &c., helps us a very little way beyond this; but that is surely a deficiency, not a superiority. The eye, unlike other organs, having different perceptions, advances beyond the ordinary limits of accurate distinction of one class of these by the help of the others. But this in fact supposes that both classes are proper perceptions of sight. What is imagined and estimated is *visible* distance, as the laws of reproduction require.

It so happens however that the sense of touch furnishes us with a precisely similar instance. That by touch, or rather the muscular sense, we perceive weight, is undoubted. Slight differences of weight, however, cannot be recognised in this way; but a person accustomed to handle and to judge the weight of any particular substance, *e.g.* of paper, as a post-office clerk does that of letters, will be aided in his judgment by the bulk; and in estimating minuter differences will sometimes be misled thereby. The influence of other visual signs is illustrated by the anecdote connected with the first discovery of potassium. When this substance was first shown to Dr. Pearson he exclaimed, "Why, it is metallic;" and then balancing it on his finger, added, "Bless me,

how heavy it is!"* The fact being that it was incomparably lighter than any metal then known—lighter even than water.

But shall we therefore conclude, that when we balance an object in the hand our judgment of its weight is only the result of an association with bulk and other visual signs? It would be a rash inference, yet not a whit more rash than on the like grounds to deny to sight the original perception of distance. Within its own limits the eye is as little liable to be deceived in measuring distance as the touch in measuring either weight or hardness.

But what if the received theory is wholly incompetent to account even for the facts to which it appeals, the errors or illusions of sight? It is on these it is compelled to rely for verification; and if it cannot explain these it is good for nothing. We may expect, then, that it will cost some trouble to invalidate the evidence derived from this source. The difficulty however is a negative one. It is so far from being established that the limits and variations of the visual perception of distance correspond to the limits and variations of the suggesting and suggested ideas, that there has not been even an attempt to show such a correspondence. Let us see whether there is any indication of its existence.

It is a very general law that the distinctness of two (measurable) perceptions depends, not on their difference, but on their ratio.† For example, if we can just distinguish two weights, each about an ounce, but differing by a fortieth part, then, if we multiply these weights tenfold, we shall find just the same difficulty in distinguishing them. If,

* Paris's "Life of Sir H. Davy," vol. i., p. 268.

† This law was discovered by E. H. Weber, and is known as Weber's law. Fechner has confirmed and extended it.—"Psychophysik," vol. i., p. 134; vol. ii., p. 565.

again, we try to compare two lines of nearly the same length (not seen together), we shall find that the absolute difference is of no consequence; the question is, what proportion it bears to the whole length of the lines. The same is true of the measure of distances by locomotion. The difference between three miles and four is as sensible as that between three yards and four, or between six inches and eight. The same law holds with respect to degrees of light and distinctness; the least difference of brightness which can be perceived being about one-sixtieth. Hence it follows, that if distance as seen is a suggested idea of locomotion, great distances must be seen with the same relative accuracy as small; and the difference between three miles and five must be seen more vividly and distinctly than that between twelve inches and fifteen; the difference of exertion suggested in the former case being much greater than in the latter, and the difference in the distinctness and brightness of the objects seen being also greater beyond all comparison. And since, on the same theory, apparent magnitude and apparent distance are inseparably connected, we must be able to estimate the magnitude of an object with the same accuracy, whatever its distance may be. Now the fact is, that there is no comparison between the accuracy with which near and remote magnitudes and distances are estimated. Beyond a certain small limit increase of distance is not seen at all. We are able indeed to estimate greater distances by means of various signs, amongst which the series of intervening objects or the intervening ground holds an important place; but we never imagine that we see a difference of distance between objects at two and at five miles distance in the same sense that we believe we see the distance of near objects.

Again, the suggested thought or judgment may be altered while the perception remains the same, and *vice versa*. Thus

inexperienced persons make very bad guesses at the real distance and magnitude of remote objects; whereas a practised traveller learns to make allowance for a variety of indications which either escape or mislead the former. Now, if Berkeley's theory is true, this acquired skill must of necessity be accompanied with a proportionate change in the apparent distance and magnitude. Estimated and apparent distance are in fact, on this theory, convertible terms. But we find, on the contrary, that these are the same at the end as at the beginning of our experience. In the Alps, for example, a glacier which is some miles distant may (if there are no intervening signs of distance) seem only a few hundred yards off. A short experience teaches us to correct this under-estimate; but to the last the appearance believed to be seen remains the same.

But we have as yet scarcely spoken of the suggesting ideas. They deserve however a closer examination. It has been so constantly affirmed that distinctness and brightness are the signs by which distance is suggested, that the correspondence has come to be taken for granted as a thing beyond question. The fact is however that the only trace of such correspondence is in our estimate of great distances —that is to say, of those which are admitted to be out of the range of direct perception. To found a general theory on such extreme cases is obviously unphilosophical. But the theory of suggestion by brightness fails even in its application to these. We do not find that the apparent distance and height of a mountain vary in proportion to its distinctness and brightness. Reid admits that it is only in extreme cases that there is any such variation, viz., in the case of a fog on the one hand, and of an extraordinarily clear atmosphere on the other. We learn, he thinks, to make allowance for all ordinary differences. But surely if we make allowance in any case, it ought to be that of a fog. We cannot allow for a difference which is yet unknown: now, most of

the variations in the transparency of the atmosphere, even when considerable, are inferred only from the appearance of distant objects; a fog or mist is unmistakably known from those at hand. It will be found too, I believe, that brightness is often conjoined with an increase in the apparent distance, and dimness with a decrease. In particular, it may be noticed that a mountain looks larger when we can see some considerable inequalities and differences of light and shade on its surface than when only the outline can be discerned. When a mist is so dense that the apparent magnitude of a mountain is increased, instead of appearing farther off it seems nearer, sometimes almost overhanging. Lastly, the summit of a tower projected against the sky appears by contrast less bright than the base—an effect represented in drawings by deeper shading; yet the apparent distance of the summit is not increased.

One fact is indeed sufficient to overthrow the theory of suggestion by degrees of brightness. It is an obvious consequence of this theory, that our discrimination of distance must be more delicate in proportion as the decrease of brightness is more rapid, that is, as the air is less transparent. In very clear weather we should be always at a loss, while in a thick haze our perceptions should be particularly accurate. Experience shows, on the contrary, that the clearer the atmosphere the better we can distinguish differences of distance, as well as all other objects of sight. It may possibly occur to the reader that we have an exception to this in the fact just alluded to, that in a very clear atmosphere we under-estimate the distance of remote mountains, &c.; but this fact has really nothing to do with the principle now stated. The error in this case is not in the present discrimination of unequal distances, but in the measure of them; in other words, in their comparison with former perceptions under different atmospheric conditions. If

there be any direct perception of distance it is independent on this comparison; if there be not, then the comparison is the sole foundation of our estimate; and the illusion spoken of will not be confined to remote objects, but will equally affect the apparent distance and magnitude of those which are only a few yards or feet distant, and our judgment of such distances would be as fluctuating as the hygrometer.

But even if the suggestion theory accounted perfectly for our judgment of great distances, this would be no reason for applying it to small intervals; such as are measured by feet and inches. Our perception of these is, to say the least, more accurate, and is the foundation of the perception of those which are more remote. The problem to be solved in the first instance then is, how we discern the distance of near objects, which it must be confessed we do with great exactness whether they are known or not. To speak of distinguishing near intervals by "aerial perspective" is simply absurd, as an easy calculation will demonstrate. It must be remembered that the least discernible difference of brightness is one-sixtieth; and it will no doubt be granted that we can distinguish clearly and certainly the interval in depth between two objects distant respectively six and ten inches. The loss of brightness in these four inches must therefore be $\frac{1}{61}$ at least. If now the diminution of light in passing through the air were progressive, the same proportion being lost in each foot traversed, then at this rate the light of the sun would be reduced in less than one hundred yards much below that of the moon, and in twice this distance below that of a fixed star. This law however (which is Bouguer's, supported by the observations of Pouillet) does not appear to be accurate. According to the more exact researches of Professor J. D. Forbes,* the loss of light follows a law analogous to that of the transmission of heat through glass. Heat which has

* Philosoph. Trans., 1842, p. 225.

passed through one glass plate will pass more readily or with less relative loss through a second, with less still through a third, and so on; so that when a certain number of plates has been passed through, the remaining heat will not be sensibly diminished by any further increase in the thickness of the glass. By a similar law, when solar light traverses air, about one-fifth of it may be regarded as not liable to extinction at all, while the remaining four-fifths are subject to a progressive diminution, as in Bouguer's law. Thus the light tends to fall to one-fifth of its original intensity, but not lower. Now, according to Forbes's observations, the loss of light in passing vertically through the atmosphere or horizontally through six miles is about one-half. This gives in one yard a loss of about $\frac{1}{12000}$;* and there would be no sensible difference in brightness for a distance of nearly a mile and a quarter.

If an interval of four inches caused a sensible difference of brightness, as before assumed, then by Forbes's law the brightness would be sensibly reduced to its minimum in one hundred inches, beyond which it would be unaltered.

The faintness of remote objects, however, in the ordinary state of the atmosphere appears to be chiefly owing to a different cause, namely, the intrusion or intermixture of light reflected at various points in the intervening air. The laws of this atmospheric reflection are yet unknown, but experience shows that the loss of clearness and relative brightness from this cause is as insensible at small distances, and increases as rapidly at great distances, as that from imperfect transparency. The absolute brightness, however, of the distant

* This is on the assumption that the light reflected to us has not previously lost anything by absorption. But as the solar light has already lost one-half when it reaches the surface, only $\frac{3}{5}$ of the remainder is liable to progressive extinction; the fraction above given might therefore be reduced to $\frac{1}{17000}$. The hundred inches given below would be reduced by the same consideration to fifty-two.

field of view is, on the whole, increased by this reflection. This phenomenon, which is generally overlooked by optical and meteorological writers,* is well seen in photographic views of distant scenery, the sensitive plate being in fact a self-registering photometer.

Besides the decrease of brightness a decrease in distinctness is mentioned by Mr. Mill and Mr. Mansel as helping to suggest distance. If by this they mean the indistinctness owing to the cause just mentioned, then we may apply the same argument that has been used with reference to brightness; but they seem rather to mean that want of clearness in detail which arises from the smallness of the retinal image. In this sense however indistinctness is wholly relative to our previous knowledge of the object; and, like apparent magnitude, it can in no way help to suggest the distance of an unknown object. This use of the words "distinct" and "indistinct" appears to be improper. We do not say that we see a leaf indistinctly, because we cannot distinguish the details which a microscope discovers to us; nor is our vision of a distant tree indistinct, because we cannot see the leaves which would be visible on a nearer view. Vision is properly called distinct whenever the retinal image is perfectly defined. With respect to this, recent writers, although using the language of Berkeley, have departed widely from his meaning. In his sense of the words "confused vision," which he opposes to "distinct," is "when the rays proceeding from each distinct point of the object, are not accurately re-collected in one corresponding point of the retina, but take up some space thereon" (sec. 35). But this only concerned objects too near to be seen distinctly within this distance; "the greater confusion still implying the lesser distance, and the lesser confusion the greater distance of the object" (sec. 21). Beyond this very

* Bouguer is an exception; but his results are obtained by calculation, not by direct observation. "Traité d'Optique" (1760), p. 360.

small limit, and within the whole range, therefore, of distinct vision, there was according to Berkeley no suggesting idea but faintness, and the foregoing considerations are sufficient to show its utter inadequacy to account for the phenomena.

Nor is the case mended by supposing, as Sir William Hamilton does, that the tactual perceptions are suggested by the sensation of the muscular adaptations of the eyes—adjustment and convergence; for when the eyes are shut, or in the dark, these sensations do not suggest distance. On the contrary, in the absence of visual sensations it is only by imagining them that we can cause the eyes to assume the corresponding adjustment. Further, when by the use of lenses we alter the convergence of the rays, and with it the adjustment, it is not found that the distance perceived is altered in like proportion. And lastly, it is not true that when we look at one object, all others (seen now with the same adjustment) appear at the same distance. On the contrary, their apparent distance is the same, approximately, as when they are seen with the appropriate adjustment.

CHAPTER IV.

PHENOMENA INEXPLICABLE ON THE BERKELEIAN THEORY.

I SHALL now mention two or three phenomena which have never been explained on the common theory, in addition to those which have been already noticed in passing. First occurs that of single vision with two eyes. One set of philosophers cut this knot by denying that any explanation is required. Instead of looking at the phenomenon which is actually present they substitute a different one, in order to fit their hypothesis. It is a fact, that in vision with one eye we distinguish a vast number of apparently identical points, assigning to each a definite position in space. In other words, when images are formed on two or more different parts of our retina we see the objects in as many distinct places whether the object is one or many.* Every object seen by either eye is thus seen in a definite place. Now the phenomenon in question is, that in certain circumstances when seeing with both eyes we refer to the same place in space objects which affect different parts of the compound organ. This is a phenomenon, says Mr. Bain, of the same kind as single hearing with two ears, or single smelling with two nostrils. "Inasmuch," says Mr. Lewes, "as the object affects both eyes *simultaneously* and with *equal intensity* it cannot produce two sensations, but only one sensation."† He supposes that it may be objected that a drunken man may see a single object double, and explains this fact by suppos-

* On double vision with one eye, see Reid, Essay II., sec. 12, p. 161.

† "Physiology of Common Life," vol. ii., p. 339.

ing a want of simultaneity in the action of the nerves produced by irregularity of the circulation in the brain. We shall see presently that the simultaneity of the impression is not of the slightest consequence. Indeed it is obvious that a difference in time cannot account for a difference in place. Can it be possible, however, that Mr. Lewes is not aware of the fact that a single object may be seen double without any disorder of the nerves, if only the visual axes converge to a point a little more or less remote? For example, let a pencil or penholder be held so that the nearer end touches the bridge of the nose, the further end being somewhat depressed, two distinct images will then be seen; and the point of intersection may be moved at will by changing the convergence of the axes. If the pencil is so placed that one side brightly reflects the light, the contrast of the two images will be more striking, but of course their distance will not be increased. And on the other hand, two different objects may be made to appear in the same place by a suitable convergence of the visual axes. But it is indeed needless to have recourse to experiment. If two simultaneous and equal impressions can produce but one sensation, it would be impossible to see two precisely similar points or objects of any kind as two. A sheet of postage stamps looked at fixedly would appear as a single head; a thousand sovereigns fresh from the Mint must be seen as one, and every one we meet would be in our eyes a Cyclops, if not also one-armed and one-legged. The more symmetrical his form the more certainly must he appear a "monstrum horrendum informe." Happily, Nature has been kinder to us than these philosophers would have her, and has given us powers which transcend their conceptions of possibility.

In referring to the analogy of the other senses, such as hearing and smelling, Bain, Lewes, Nunneley, &c., have not seen that the point on which the whole difficulty turns is

precisely the point in which sight is contrasted with these, which do not perceive external things as in space at all. If two sounds are simultaneous and alike in pitch, tone, intensity, &c., there is nothing left by which they can be distinguished. But with the eye it is otherwise. Objects quite undistinguishable as to all their attributes are distinguished by a difference of place.

It is curious to find Leibnitz's principle of indiscernibles re-appearing under this form in a totally different school. According to this principle there cannot be in nature two perfectly similar things having the same identical qualities and quantities; for if so, they could not be distinguished as two. It is true indeed they could not be distinguished by means of concepts, but by intuition they may, since they may be perceived in different places.* The same is true of things indiscernible to sense; although perfectly alike in quality they can be distinguished in place. Mr. Lewes is therefore mistaken both in his facts and in his philosophy.

Mr. Mansel again admits the difficulty, but thinks that the phenomenon in question is simply a matter of custom or association—that we have been accustomed to find two ocular images in certain circumstances corresponding to one resistance or one perception of resistance, and that therefore we see them as one. This would surely never have been imagined by so acute a metaphysician but for the force of Berkeley's theory. Every case which can be supposed as in any degree analogous will be found to tell against it. First however I would ask, what is "one resistance?" The term required by the explanation is "one resisting body;" but

* Reid, "Inquiry," chap. 6, sec. 17, pp. 174–5. Kant, Kritik d.r.V. Ed. Hartenstein, p. 254. (Meiklej. Trans., p. 202.) The principle as held by Leibnitz himself is not exposed to this objection. On the history of the principle cf. Platner, "Philosophische Aphorismen," vol. i., p. 516, sec. 888, (1st ed.)

this Mr. Mansel has doubtless avoided using, because the oneness of the body is an inference, not an immediate perception of touch. But by so doing he has substituted a false account of the phenomenon. "One resistance" can be felt only when we touch but one spot of a body. If we wish to ascertain the shape, we touch, successively and simultaneously, several parts of it; the continuity of the impression when the hand is moved is the condition of the conception of its unity. But this is widely different from one perception of resistance.

Secondly, although the double image is associated with and suggests a single body, that is no reason why it should cease to be seen as double. As much might we expect that a familiar echo suggesting a single sounding body should therefore be at last heard as a single sound. If successive sounds cannot be heard as one, neither can locally separated objects be seen as one. Mr. Mansel's explanation ignores the very fact which creates the difficulty; namely, that the objects of sight are seen as locally distributed, at least in superficial extension.

Thirdly, in every case in which it can be proved that we ever saw double, we always continue to do so. For instance, when a near object is distinctly seen by both eyes while the visual axes converge to a point either beyond or on this side of it, it will, if attended to, be seen double. Other similar instances are enumerated by Reid, who remarks:—"I have amused myself with such observations for more than thirty years; and in every case wherein I saw the object double at first I see it so to this day, notwithstanding the constant experience of its being single. In other cases where I know there are two objects, there appears only one, after thousands of experiments. Let a man look at a familiar object through a polyhedron or multiplying-glass every day of his life: the number of visible appearances will be the same at last as at

first, nor does any number of experiments or length of time make the least change." He concludes with justice that phenomena so invariable and uniform in all men, and so exactly regulated by mathematical rules, are not the effect of custom but of fixed laws of nature.* I have myself been for many years in the habit of making similar observations, and with the same result. In particular, from an inequality in the focal length of my eyes, which interferes with their concurrent action, it happens that when I look at a distant object, especially if it is luminous, I see it at first as double, although by a muscular effort I can unite the images. This has been the case for at least twenty years, yet I do not find the slightest tendency to see a star, e.g., single in any case where I formerly saw it double.†

Indeed so powerless is custom in the matter that where from the form of the eye (viz., in hypermetropia) the effort to see an object distinctly is attended with an excessive convergence of the axes, the effect is, not that harmony arises between the points "accustomed to act in concert," but that, in order to avoid the double image, the one eye (in which the image is not central) turns still farther inwards, and a convergent squint is produced. On the other hand, it is owing to a similar instinctive retention of single vision that shortsightedness is not always accompanied with a divergent squint.‡

And again, persons who have always squinted, on directing their eyes to the same object see it single; and in every case in which persons born blind have obtained the sight of both

* Ibid. sec. 17, p. 176.

† "In sensorial affections, accompanied with strabismus, displacement of the globe, &c., double vision is produced and continues as long as the cause lasts, in spite of our knowledge that the object seen as double is really single."—"Lawrence on the Eye," p. 60. But see Donders, ubi infra, p. 130.

‡ Donders, "Refractie-anomaliën oorzaken van Strabismus." Verslagen en Mededeel. der K. Akad. d. Wetensch. (Amsterd.) pt. 15, pp. 134, 165.

eyes, they have seen objects single from the first.* If it were otherwise, their apparent field of view would at first have double the extent to which experience has in us reduced it, since its extent depends on the number of distinct sensitive elements. And while they were learning to identify these sensitive parts two and two, what inextricable confusion would arise!

Single vision is indeed only the extreme case of a general law for which both the theory of habit and that of identity of sensation are wholly incompetent to account. If (with Reid, Müller, &c.) we call those points in the two retinas corresponding which would coincide if one retina were superposed (without turning) on the other; then when the images of an object fall on corresponding points it is seen single, and whenever it is seen double, the images fall on points which do not correspond. Now in this case the apparent distance of the two appearances is the same in the same circumstances to all persons. It is proportional to the deviation of each image from the points corresponding to the other, in other words, to the interval which would be included between the two images if one retina were superposed on the other; and the image belonging to the right eye appears to the right hand if the visual axes meet on this side of the object, and to the left if they meet beyond. In this respect, then, the two retinas behave as one. Any explanation therefore of the coincidence of the appearances in single vision which does not also account for the law of their separation in double vision is unworthy of consideration. Both the theories mentioned are consequently put out of court.†

* For the case of strabismus, see Reid, Works, p. 172; Franz, Med. Gaz. 1840, ii. p. 540; on the recovered blind, Bortolazzi, "Sopra una cieca nata guarita," p. 93; also Cheselden's case, and Trinchinetti's, mentioned below.

† This law was given by Reid, "Inquiry," chap. 6, sec. 13, p. 165. It is not creditable to psychology, claiming to be a science of observation, that

But I must not quit this subject without noticing a case supposed to be analogous, and adduced as such by nearly all authors who have written on the question. If we cross the first and second finger, and place a pea between them, we appear to feel two peas, whereas in the ordinary position of the fingers, although the sensation is equally double, we feel but one. As "habit has always taught us that two convexities so felt belong to one and the same spherical surface we never hesitate or question the identity of the globe or the unity of the sensation."* Similarly, if a pencil is placed between the lips in their ordinary position it is felt as one, but if one is protruded more than usual the object is felt double.† "We might then with equal reason ask why, having ten fingers with equal sensibility, do we not feel decuple, as why with two eyes we do not see double? The answer is the same in both cases; it is a matter of habit." Now, in order that these cases should be parallel, we ought with each finger to *feel a complete globe.* From two convexities so felt we can have the idea of a globe only on condition that they are *not* identified. In fact, the objects of the supposed double or decuple touch are always necessarily double or decuple. With one finger point we can feel but one point of the body, but "one resistance," and with ten fingers ten different points. From a certain combination of resistances we infer that we are pressing a doubly convex figure; and if a small globe is held in the hand we combine the several resistances to form the idea of one object—a globe.

theories of single vision continue to be defended without the slightest reference to such a fundamental law, which, if forgotten, ought to have been long since re-discovered; as it was in Germany by (?) Meissner (1854), to whom Cornelius attributes it. Meissner, "Beiträge zur Physiologie des Sehorgans." Cornelius, "Theorie des Sehens," p. 365.

* Sir J. Herschel, "Treatise on Light," sec. 361.

† Czermak, Sitzungsberichte der k.k. Akad. der Wissensch. zu Wien, vol. 12, p. 314.

Thus instead of the several resistances being identified they are added together; their plurality, so far from disappearing, is the condition of the resultant idea. If it were a fact that these combined sensations appeared to the mind as one, this would simply prove that by Touch or Resistance we do not discriminate locally distinct sensations as accurately as by sight;* for that the several parts of one object are seen as several is undoubted.

The analogy therefore halts on all its legs. The sensations originally distinct are never identified, and if they were we should not perceive a solid figure at all, but a single resisting point.† The true analogue of this case is the vision of a continuous figure with one eye. If we see two opposite quadrants of a circle, the intervening space being covered, they at once suggest the idea of a circle; or if we see that the part of a surface turned towards us is globular, we form in the same way the conception of a globe. But this phenomenon has clearly no resemblance whatever to that we have been considering. I think then that I am justified in affirming that no explanation of the phenomenon of binocular vision consistent with Berkeley's theory has yet been found.

Another very familiar fact deserves also to be mentioned for the same reason; namely the judgment we form of the apparent magnitude of the sun and moon and of intervals in the heavens generally. Mr. Mill speaks of these judgments

* That this is the fact, is easily proved by placing the hand on a number of balls, or still better, pencils, or laying them on the hand. It will be found that it requires a considerable effort of attention to distinguish accurately even a small number.

† Compare Graves, "Studies in Physiology and Medicine," p. 224, where a similar view is taken of this supposed analogy. See also E. H. Weber, in Wagner's Handwörterb. Art. "Tastsinn," p. 541. Mr. Lewes's explanation of the phenomenon is, that the nerve of one finger being compressed, the impression is deadened and retarded. How is it, then, that two balls held in the two hands are not felt as one?

as the fancies of children. No doubt, if his views are correct they ought to be only the fancies of children. But from all that I can learn, the experience of others agrees with my own, that not children alone, but all persons at all times see the moon, for example, as "no larger than a cheese," and the length of Orion's belt as not much more than a yard; and that no knowledge or reflection upon the fact of their great distance alters or tends to alter this impression; and further, that the judgment of different persons with respect to these magnitudes is remarkably uniform, varying only and inconsiderably with certain external conditions.

Explicit testimony with respect to this question is hardly to be looked for in the works of philosophers; but such testimonies as I have found are remarkably uniform. Thus Heraclitus held that the sun was a foot in breadth.* Aristotle, observing that some things appear to us otherwise than we know them to be, gives as an instance that the sun appears to be only a foot broad although we are persuaded that it is larger than the earth.† Cicero again, arguing that the senses are fallacious, remarks, "What can be larger than the sun, which mathematicians prove to be eighteen times as large as the earth; how small it appears to us! To me it appears about a foot in breadth."‡ Arthur Collier, in attempting to prove that visible objects have no external existence, remarks that "the moon which I see is a little figure of light no bigger than a trencher."§

* *εὖρος ποδὸς ἀνθρωπείου.* Plutarch, Plac. Phil., lib. ii., 21.

† *φαίνεται δὲ καὶ ψευδῆ, πέρὶ ὧν ἅμα ὑπόληψιν ἀληθῆ ἔχει· οἷον φαίνεται μὲν ὁ ἥλιος ποδιαῖος· πέπεισται δὲ εἶναι μείζω τῆς οἰκουμένης.* De Anima, lib. iii., cap. 3, sec. 10.

‡ Quantulus nobis videtur! mihi quidem quasi pedalis. Acad. Prior., lib. ii., cap. 26. To the same purpose Seneca, "Hunc [solem] quem toto orbe terrarum majorem probat ratio, acies nostra sic contraxit ut sapientes viri pedalem esse contenderint. Quæst. Nat. i., 3.

§ Clavis Universalis, Pt. i., cap. i., sec. 2, in "Metaphys. Tracts," p.

This fact, which is only the most striking of a class, appears to be wholly inexplicable on the received theory, according to which apparent magnitude depends on the distance suggested or conceived. Mr. Mill is clearly of the same opinion, since he thinks it necessary to ignore the fact.

A third stumbling-block to the received theory is the phenomenon of the variation of apparent magnitude. The ambiguity of the term itself has given rise to much confusion; since "apparent" is sometimes taken in a psychological, sometimes in a mathematical sense. In the latter, apparent magnitude is the angle under which an object is seen. It is constantly assumed that this (which I shall call angular magnitude) is the measure of visually or sensibly apparent magnitude. Yet this is in manifest contradiction to experience. A pencil has the same apparent magnitude at four inches as at forty, and we cannot by any effort see it as one-tenth of the size at the latter distance. If we hold a pair of compasses at a distance of twelve inches from the eye, and having carefully observed the interval between its points, open it again successively at half and at double the distance, doing our best to give the interval the same visual measure as before, it will be found that the actual opening will be nearly the same, instead of being the half and double respectively.* He that gathers samphire half-way down the cliff may seem "no bigger than his head," but at twelve yards he seems no smaller than at two; yet it is at twelve yards that he is mathematically no bigger than his head at two. In the former case, our knowledge or estimate of the distance does not enable us to see him in his full size; in the latter, on the contrary, our knowledge of optics does not

23. He adds, "Nay, so little as to be entirely coverable by a shilling." Coverable, viz., because its angular magnitude is less than that of a shilling (held in the hand), although it appears much larger.

* Cf. Fechner, Psychophysik, vol. ii., p. 311.

help us to see what we are taught is the visible magnitude. In general, the apparent size of near objects (or within the range of distinct vision) is the same however their distance may vary, while the apparent size of remote objects diminishes regularly as their distance increases, no matter what our estimate of the distance may be; and similarly, an object within the nearest limit of distinct vision seen through a pinhole, or otherwise, appears enlarged. Now, if (sensibly) apparent magnitude depends solely on the angle subtended, a man at six yards appears as small as an infant at two; if, as Berkeley holds, it depends on the angle combined with the degree of brightness, then he ought to seem no less at two hundred yards than at twenty. Either the constancy in the former case or the diminution in the latter is unintelligible. A comparison of the two cases shows that visible magnitude must be distinguished both from estimated and from angular magnitude.

There remains one very important phenomenon, which on Berkeley's theory is utterly mysterious and inexplicable. In order to see distinctly objects at different distances the eyes require to be adjusted by a certain muscular effort, to be presently specified, precisely as we adjust a microscope by the motion of a screw. Now, if distance (within the limits of this adjustment) were suggested by degrees of brightness, &c., the adjustment itself must be also dependent on these sensations; and as they are neither very definite nor accurate signs the adjustment suggested would be often unsuitable. In fact, erroneous estimates of distance, the illusion of paintings, &c., would be always accompanied with imperfect focussing. When trying to look at an object we should constantly find it indistinct and double; and should be obliged to ascertain the right adjustment by a slow and tentative process, as in the case of the microscope. Indeed the very ideas of indistinctness (in Mr. Mill's sense) and of relative brightness

presuppose that the object is already "looked at" or brought into focus. Everything out of focus would seem very distant.

Now we find, on the contrary, that neither brightness nor indistinctness (in this sense), nor yet apparent magnitude, has the slightest influence on the adjustment of the eyes, which is moreover effected with such quickness and accuracy that the moment we will to look at any object it is seen distinctly and single without any groping or failure whatever; so that unlearned persons actually never notice either a double image or the indistinctness of an object in the line of sight. In other words, the organ which is alleged to be insensible to distance does nevertheless exhibit a spontaneous change exactly proportionate to the variation of this unknown condition, and independent even on our ideas of it. This single fact is sufficient to overthrow the whole theory.*

* The insignificance of brightness and distinctness in the perception of distance was fully proved from experiment by Dr. R. Smith, in his "Complete System of Optics" (Rem. 197, seqq.) It ought to be unnecessary to prove over again what was established more than a century ago in a book of the highest authority, and by proofs which have never been questioned. Smith himself thinks that apparent distance is suggested by apparent magnitude; but most of the arguments above adduced apply as well to that supposition as to Berkeley's.

CHAPTER V.

IS TOUCH PERCEPTIVE OF EXTENSION AND DISTANCE?

We must now proceed to assail the theory in its foundation —the assumption that Touch is the sense pre-eminently perceptive of extension and distance. And here, again, we have no really philosophical theory to combat; nothing is opposed to us but vague and popular impressions.

Touch proper gives us nothing but a series of sensations which have of themselves no more connexion with extension than with colour. Brown's attempt to educe extension from the muscular sense, has been so conclusively exposed by Sir W. Hamilton that it scarcely needs mention. It is sufficient to say, that a series of sensations, muscular or otherwise, is nothing but a series of sensations which take place in time, and may therefore suggest that idea, but have no relation whatever to space. However, as an attempt has been lately made to derive extension from the locomotive faculty, I shall devote a short space to an examination of this theory.

Mr. Bain's theory of the nature and origin of our notions of externality and distance may be gathered from the following passages:—"Our belief in the externality of the causes of our sensations means that certain actions of ours would bring the sensations into play, or modify them in a known manner."* "Any impression that rouses a stroke of energy within us, and that varies exactly as that energy varies, we call an outward impression."† "The coalition of sensations of sight and touch with felt motive energies explains every-

* "The Senses and the Intellect," p. 373. † Ibid, 371.

thing that belongs to our notion of extended magnitude in space."* "The association between sight and locomotion, or between touch and the movements of the arm, tells us that a remote building implies a certain continuance of our walking exertions to change its appearance into another that we call a nearer view."† "The feeling we have when the eyes are parallel and vision distinct is associated with a great and prolonged effort of walking, in other words, with a long distance."‡ For smaller intervals, "the material of recollection of one foot is an arm impression, just as the material of recollection of greenness is a visual impression."§

According to Mr. Bain, then, we find that a certain exertion of the muscles of the arm enables us to reach and touch certain objects; a little increase of exertion is necessary for others, and less for those that are nearer. Now it is plain that we can obtain from all this nothing but the idea of muscular exertion. This Mr. Bain appears to admit, and he maintains that our idea of distance is nothing else. If Mr. Bain really has no other idea of distance or depth, I do not see how he is to be convinced that that of most men is different. Indeed if it were certain that a man born blind has no other idea of distance than that it is something to be

* Ibid, p. 368. † Ibid, p. 369. ‡ Ibid, p. 371.

§ Although not directly treating the question of Realism, I cannot help remarking the singularity of a theory which resolves our perception of the external into a consciousness of our own energy. Certainly, a blow or a knock which rouses no energy gives us as decided a sense of the external as the motion of the hand in lifting a weight. The first passage cited is most extraordinary. It may be fairly paraphrased: "Our belief that our sensations are produced by something without us means that they are produced by ourselves." After such a statement it is scarcely worth while to observe, that the fact to be accounted for is not a *belief* that the causes of our sensations are external, which statement might apply to the more subjective senses of hearing and smelling, but a belief that we perceive, see, and feel the causes themselves when we are not conscious of any sensation.

overcome by a certain amount of exertion in a certain time, this would be a very interesting fact; but so far from proving Mr. Bain's theory, it would be all that is required to prove that touch cannot give any idea of extension. The two ideas are as distinct as those of colour and time. "It is true," says Reid, "we have feelings of touch which every moment present extension to the mind, but how they come to do so is the question; for those feelings do no more resemble extension than they resemble justice or courage."* But we must examine the subject a little more in detail.

Since motion cannot suggest the idea of anything external we must consider it in connexion with touch. Now touch itself cannot give the idea of anything beyond or without us, since for touch nothing has any existence which is not actually in contact with the surface of the body. Any part of this surface may be affected independently, and apart from the rest; it may therefore be considered as a collection of organs, each tactile minimum or unit being a distinct organ. But we are not aware even of the existence of any part except so far as it is actually sentient, nor have we then any consciousness of its place. To see this clearly it is only necessary to clear up the signification of the terms. Place is a relative idea; by the *place* of my toe, for example, I mean its position in space relatively to the other parts of the body. To perceive this relation all these parts must be also perceived. It is impossible therefore that a single nerve or sentient part should convey the complex perception of this relative position. Nor have we any knowledge of this even when the parts compared are in activity together. We are not conscious of the size of the toe, or its form, or its distance from the centre. All sensations are in fact central, and it is only by association that we learn to refer certain sensations to certain external visible and tangible organs. The word

* "Inquiry," chap. 5, sec. 5. Hamilton's Ed., p. 124.

toe, for instance, expresses three distinct ideas. It means the object of certain perceptions of sight, of certain perceptions of touch, and thirdly, the seat of certain sensations; in Berkeley's language, the visible, tangible, and sensible toes. But we have no *a priori* knowledge of the correspondence of these perceptions and sensations. The part is known to consciousness only as the seat of certain sensations; we no more know by intuition that it will present such and such an appearance to the external senses, than we know the shape of the heart or the anatomy of the nervous system by intuition. If we had never seen or handled our foot, no amount of pricking and pinching would enable us to draw the outline of it, or guess at its distance from the body. But as we learn by association to identify the toe felt with the toe seen, so it is also by association that we identify both with the toe feeling. By touch I find that when impressions are made on any part of a certain surface I feel them; and I learn to regard this as the surface of my own body. In particular, when I touch the aforesaid visible and tangible toe, certain specific sensations are excited, and thus I come to refer these by association to that part of the visible and tangible surface. When an impression is made on the less practised or less percipient parts of the body, it is only by renewing the impression with the finger that we can judge accurately where "the shoe pinches."

And since external pressure on the chest, abdomen, or head, often produces besides the skin sensations a second distinct from it, and which must therefore be conceived as below the surface, so when these deep sensations occur alone they are of course referred to the same place as the corresponding skin sensations with which they have been associated.

When I say then that I feel a pain in my foot, I am not stating a fact of consciousness but an inference. The part in which I feel the pain is known to consciousness only as the

seat of such sensations. I infer from association that the source of the pain is in a certain visible tangible part of the body. But for this it is not at all necessary that the foot itself should be sensitive; it is enough if impressions on it give occasion to distinct sensations. Thus when we press a walking-stick on the ground we seem to feel two sensations in places separated by the length of the stick. We feel the resistance of the ground propagated through the stick, as well as the contact of the stick itself with the hand. If the stick is fixed immovably to the ground the former sensation disappears altogether. On the other hand, as E. H. Weber remarks, if we could fasten the stick immovably to our hand the sensation of contact would disappear, and there would remain the pressure, which would appear to be felt at the lower end of the stick.* This is exactly the case with the teeth, which are fixed in the jaw. A pressure on the tooth is referred to its free surface, and not to the place where it is in contact with the nervous surface of the socket. But when the tooth becomes loose we feel both the pressure on the surface and also in the socket.

These principles furnish the true explanation of the fact, that after a limb has been amputated sensations continue to be referred to the lost part. Since the local reference of sensations to external parts is wholly the result of association, it must follow that when the external organ is removed or altered, the old association will continue to operate until there has been time to establish a new one. And if it were possible to transfer a nerve from one part of the body to another, its original associations would subsist for some time, but would gradually give way to others arising from its new position. Now this is precisely what happens in the Talia-

* E. H. Weber, Art. Tastsinn, in Wagner's Handw., p. 484. Compare Carpenter, Art. Touch, in Todd's Cyclopædia, p. 1167a. Cornelius, p. 618.

cotian operation, when a portion of the skin of the forehead is applied to the formation of an artificial nose. At first, when the new nose is touched, the sensation is felt as if in the forehead, but by degrees the subject of the operation learns to connect his sensations with the new external organ.* This change however would be impossible if the original reference had been instinctive. The reference of sensations to external organs is indeed analogous to the recognition of a friend by his voice. If an intimate friend should die, leaving a son with a voice like his own, we could not for some time hear the son speak without fancying for the moment that our old friend was present.

If now we suppose that an external object is touched, it appears from what has been said that this sensation cannot be accompanied with a perception of the place of the object. If several sensitive minima are affected at once, then these sensations being similar, and yet distinct, are perceived as out of one another, and therefore imply the idea of extension as the condition of their distinction, but not necessarily in more than one dimension. But of the interval between them we have no idea, since this is measured by the number of distinct sensations which might possibly be interpolated; still less can we perceive their distance from the centre of sensibility. Touch proper therefore cannot give the idea of place.

Put the case now that we move the hand. Does this motion supply the deficiency of touch? By no means. It has just been shown that we have no immediate knowledge of the extremity, but this is still more manifest when motion is in question, since in this case the extremity moved is not even the external seat of sensation. I can move the hand by acting on the lever of the arm at the elbow, the muscles of

* Cf. Hagen, in Wagner's Handwörterb.; Art. "Psychologie und Psychiatrie," vol. ii., p. 716.

the forearm suffering no change, and therefore conveying no sensation. In fact, the parts beyond the extremity of the last contracting muscle are moved exactly as we should move a walking-stick, and with just as little consciousness of their motion. We are conscious only of the contraction of the muscle. This is certainly a sort of motion, although in no respect like the motion said to be perceived. But the muscle itself is not sensible; the nerves alone can receive or transmit a sensation; and in relation to the nervous system the muscles are an external substance. How the nerves receive impressions from these or any other external thing is unknown, and how the motor nerves induce muscular contraction is equally unknown; but there is no probability that motion intervenes in either case. A muscle, we know, may be made to contract by a galvanic current, and *vice versâ*, its contraction develops a current. But whether nervous action be analogous to galvanism or not its character does not depend on the exciting cause, and that particular action which determines or is determined by muscular contraction is no more motion than the sensation of colour. Muscle contracts under the influence of the motor nerves, because contraction is the special manifestation of muscular activity. Muscular motion then is not more naturally connected with the idea of motion than any other organic phenomenon.

But, further, it is even incapable of originating it; for the idea of motion implies the comparison of the place left with that reached. But to Touch these do not co-exist; they are not known together. The sensations concerned are received successively by the same nerve or bundle of nerves; they therefore imply the idea of time as the condition of their distinction. But as felt in the same sensitive part they cannot be perceived as out of one another in space. In fact, similar sensations in the same nerve cannot be distinguished except on the condition of succession in time; it is impos-

sible therefore that they should be perceived as standing in a relation which implies co-existence. It follows, that the motion of our own limbs cannot suggest the idea of space at all, if it is not otherwise given. Motion cannot be conceived without space; and for this very reason, unless the sensations are such as to involve that idea, it cannot be perceived as motion until the idea of space is already developed. But we have seen that the idea is not involved in the sensations.

Walking is still more evidently incapable of giving the idea of progress. In fact, it is owing to external accidents that the motion of walking enables us to advance at all. Let us analyze it briefly. The forces on which the motion depends are extension, resistance, and gravity. Suppose now that we are beginning a step, by raising the left foot from the ground, the right being planted in advance. The left foot then swings forward, and the body having already a forward impulse moves with it. In this part of the step there is no muscular effort, as the Webers have shown; the leg swinging simply as a pendulum by the force of gravity. The forward motion of the trunk is also assisted by gravity. As soon as the left foot reaches the ground, the right, now the hinder foot, is extended; and this extension against the resistance of the ground necessarily propels the trunk forwards. As soon as the centre of gravity comes over the left foot, the right is lifted, and the second step begins. The muscular exertion then by which we propel ourselves is simply the extension of the leg and foot; but whether the foot or the body is moved in space by this extension depends on the question, which meets the greatest resistance; and that the body moves horizontally rather than vertically is owing to its weight and the inclination of the leg. In a treadmill or in the water the alternate extension and swinging of the legs effect no progress. Even if we grant then that we are conscious of the motion of the foot, it can only

be known as change of position relatively to the centre; of an absolute translation of the centre of gravity we can no more be conscious than the occupant of a railway carriage can be conscious of his progress. The effort of walking is indeed essentially the same as that of working a pair of treadles; the only difference being the swinging which takes place in walking, and which is wholly passive. The treadles may, however, act on a velocipede, and then the effort propels us, as in walking: the mechanical action in the two cases is similar, and in both is equally unknown to consciousness. Rowing is still more analogous to walking, the leg being represented by the oar and arm combined, which compound limb is extended (by the contraction of the arm as of its muscle) against the resistance of the water. Now, it is clear that in rowing we are not conscious of the progress of the boat; but we have seen above that as far as regards the act of motion the leg is as much an external instrument as the oar.*

It being apparent then that the motion of the hand cannot give the idea of space if we do not suppose it already present, there remained nothing for the advocates of the muscular theory but to deny the idea of space altogether, or in other words, to identify it with muscular exertion. Accordingly, this is what Mr. Bain and others have done, as we have seen in the passages already cited: "the material of recollection of one foot is an arm impression." They have only involved themselves thereby in increased difficulties. I shall not argue the question on the higher psychological grounds, but shall show directly the absurdity of the reduction. Indeed the obvious differences between the two ideas are so great that

* On the theory of walking, &c., see the Webers, "Mechanik der Menschl. Gehwerkzeuge" (1836), (translated in the Encyclopédie Anatomique, vol. ii.) The results are given by Mr. Bishop in Todd's Cyclop., Art. "Motion."

a philosopher who has neglected them can hardly be convinced by any more abstruse considerations. Thus, muscular effort has degrees; its parts are not equal: extension does not admit of degrees; its parts are equal. Extension has three dimensions; muscular effort only one. The parts of extension are co-existent; those of muscular effort are successive. More particularly: the idea of muscular effort consists of a series of sensations; to compare two such series we ought to have an idea of each as a whole; but the series is never present as a whole. It is only by a deliberate and difficult effort of attention that we can retain one part of the series until the next succeeding appears. Yet this is what we have to suppose infants perpetually engaged in doing.

It may, however, be well to argue the question on less abstract grounds. Muscular effort, or motion in general, is of course not extension. The voluntary motion of the diaphragm, for example, conveys no idea of motion in space, nor do many other voluntary muscular efforts. Mr. Bain, however, contends that the motion of the hand gives, or, more correctly speaking, is the idea of distance. Suppose now that my arm meets a resistance by which its progress is impeded or stopped, I can still exert the same amount of force as before, although the effect is different. The exertion is of the same kind, and the ideas of it must be of the same kind. I am obliged therefore to conclude that resistance and incompressibility are degrees of extension. And as the only possible idea of space is one compounded of exertion and time, we may say, space$=$effort$\times$time : therefore space$=$weight. This is certainly a novelty in psychology. Further, even if locomotion could be distinguished from other muscular effort, so as to give rise to a distinct idea, it must be admitted that the distance thus perceived will be proportional to the motive effort. Now, the hand moves in circles, of which the

centres are at the shoulder and elbow, that is to say, in epicycloids; and its position of rest is about a yard from the eye. Let us then suppose a blind man trying to get the notion of distance from the motion of his hand. He finds a certain sweep of the hand brings it into contact with a desk; the distance of which is therefore represented by that effort. But it requires a greater effort to reach the eyes or the nose, and distance being = locomotive effort, it is demonstrated that the nose extends beyond the desk. The top of the head must be conceived as more remote, and the back furthest of all. In general, when we refer distances to the eye, as we habitually do, objects four inches from the eye must appear to be farther from us than those at twelve. This is another novelty. But again, since the hand moves in curves, and cannot without considerable effort be made to move in a straight line, it is also demonstrated that an epicycloid is shorter than a right line between the same points. In fact, neither touch nor locomotion can give us the idea of a right line at all. That touch cannot has been experimentally shown by E. H. Weber, and may be proved by any one on his own person; that locomotion is equally incompetent admits also of the simplest verification. But it is enough for our purpose that the idea of the rectilinear distance between two points cannot be elicited from the "locomotive" idea of the curve joining them.

Mr. Bain seems to have confounded extension and distance. The former idea we probably obtain from touch and motion, and it is demonstrable that it is also given by sight; for diversity of colours supposes a bounding line, and a closed boundary is an extended figure. But in deriving the idea of distance from motion the problem Mr. Bain had to solve was to show how we distinguish the perpendicular component of the motion, or how it is possible to know how much

of the effort is due to the increase of distance. The distinction is manifestly impossible.

Finally, if the idea of muscular effort is either equivalent to the idea of distance or the sole source of it, how does it happen that it is only certain particular muscles of the arm and leg which have the privilege of conveying the idea? According to Mr. Bain's theory we should speak with equal propriety, and in the same sense, of walking a great distance and of singing or speaking a great distance. In a word, while all muscular motion is of the same kind it is known as such only when it produces a visible or tangible movement.

It is satisfactory to find these conclusions supported by the high authority of E. H. Weber. His conclusion from numerous and careful observations is the following:—"Of the voluntary motion of our limbs we know originally nothing. We do not perceive the motion of our muscles by their own sensations, but attain a knowledge of them only when perceived by another sense. The muscles most under our control are those of the eye and the voice, which perform voluntarily motions microscopically small, yet we have no consciousness of the motion. We move the diaphragm voluntarily against the heavy pressure of the liver, &c., yet with as little consciousness of the motion. It follows that the motions of our limbs must be observed by sight or touch, in order to learn that they move and in what direction."* "To the ideas of the eye we owe it that we learn to move our limbs intentionally in predetermined directions, and that we distinguish our body from others."† The more recent researches of Aubert and Kammler not only fully confirm this result, but tend further to prove that there is not in the muscles any sense whatever of their contraction; that we know it only through the sen-

* "Berichte der k. Sächs. Gesellsch. zu Leipzig," 1852, p. 122.

† Ibid, p. 125. Compare Fechner, Leipz. Berichte, 1858, p. 75.

sation of those parts of the skin which are pressed, extended, or contracted. Accordingly they remark that the friction of our clothing is a considerable aid in judging of our motions, especially if it is close fitting. When wearing boots, &c., with which we are not familiar, we are less certain of our movements; and this is more noticeable in riding, as the eye does not then control our judgment.*

The preceding considerations are sufficient to show that Mr. Bain's theory is altogether untenable. The fact is however that even if it were demonstrably true, it would not help us in the solution of the problem which it was framed to solve. The problem is to account for the perception of distance from ourselves and specially from the eye; for it is a fact which must not be forgotten that it is distance from the eye which is apprehended by sight. Now all external objects, we are told, appear to the eye as in contact with it, or at least at no distance from it. How then do we first acquire the idea of their distance? By the motions of the hand, it is replied, which are necessary to reach any object, *i.e.*, not, of course, to bring it into contact with the eye—which would be a suicidal supposition, as well as manifestly contradicted by experience, for even infants are not in the habit of bringing objects thus to touch the eye, and if they were, it would imply a previous idea of their distance—but to touch it with the hand. Now the hand in all its motions has been like other objects apparently at no distance from the eye. At what particular moment then have we awaked to the knowledge of its distance? when did it leave the magic plane? Its motion does not imply increase of distance from the body, since it may take place perfectly well in a plane; and as we are supposed to begin with no knowledge of space, we are not compelled or disposed to refer this motion to space. Granting, then,

* Aubert and Kammler, in Moleschott's "Untersuchungen," vol. v., pp. 159, 161.

that the idea of this manual exertion is equivalent to the idea of distance, what have we gained? To be sure, the knowledge that the object was at some distance from the hand. Our eyes might have told us that at first, if we knew which was the hand and which the object. But if they appeared in the same "coloured plane,"* or at no distance before, there is no reason why they should cease to do so. Indeed without the aid of sight there appears no reason why the motion of the hand, if known as motion, should give us the idea of anything but linear extension.

It will doubtless be generally admitted that our idea of space is something wholly different from both effort and time. Nor can space be conceived to consist in two dimensions of a visible idea, and in the third of something so heterogeneous as effort. But if the preceding argument is sound, the only idea of space which a person born blind could possess, would probably be such as Mr. Bain describes, and would at least involve the idea of time. Now careful observations have been made with a view to determine this very question by Platner, a man distinguished both as a physician and a philosopher, and his results strikingly confirm this deduction.

He writes as follows:—"The examination of a person born blind, which I have instituted with the most especial regard to this question, and have continued for three weeks, has anew convinced me that touch alone conveys no knowledge of what belongs to external space, knows nothing of local separation; and in a word, the blind man perceives absolutely nothing of the external world but the existence of something effective, distinct from his own passive feeling, and in general only numerical diversity. In fact, to those born blind time serves instead of space. Nearness and remoteness mean to them

* This language is not that of Berkeley himself, who denied that a plane was any more the object of sight than a solid; but the reasoning is equally applicable to his peculiar view.

nothing but the shorter or longer time, the less or greater number of feelings necessary to attain from one feeling to another. We may easily be misled by the language of vision which such a person uses; and I was so misled myself at first; but he really knows nothing of things as existing out of one another. This I have very clearly observed, that if objects and the parts of his body touched by them did not make different kinds of impressions on his nerves of sensation, he would take everything external for one and the same, successively affecting him, sometimes more strongly, sometimes less so. In his own body he absolutely does not distinguish head and foot at all by their distance, but merely by the difference of the feelings . . . . which he experiences from the one and the other part (and his perception of such difference was incredibly fine), and moreover through time. In like manner in external bodies he distinguished their figure merely by the varieties of impressed feelings; inasmuch, for example, as the cube by its angles affected his feeling differently from the sphere."* Neither Platner nor Sir W. Hamilton (who cites this passage), seems to have suspected that his own idea of space corresponded to this description.

* Platner, "Philosophische Aphorismen" (2nd ed. 1793, vol. i., p. 440), quoted by Hamilton, Lect. vol. ii., p. 174. See also Hagen, Art. "Psychologie und Psychiatrie," in Wagner's Handw. vol. ii., p. 718, who reaffirms Platner's statement, but without stating his authority.

CHAPTER VI.

SIGHT THE SENSE PROPERLY PERCEPTIVE OF DISTANCE OR TRINAL EXTENSION.

To sum up this portion of our inquiry: in the organ of touch or of locomotion a determinate distance is not accompanied with a determinate sensation either in the case of distance reached by the hand, in which the effort depends more on the direction than on the distance, nor in the case of greater distances reached by walking; since the amount of effort not only depends on many circumstances besides the distance, but is distributed over a greater or less interval of time, and the sensations consequently are both indeterminate and not present as a co-existing whole.

We shall now see that in the case of the eye, and the eye alone, there is a determinate sensation or state of the organ corresponding to a determinate distance of the object; and this, as before remarked, is the sufficient and necessary condition of the perception. The only possible disproof on physical grounds of the perception of distance by the eye would be a proof that the affection of the organ is or may be the same when the distance supposed to be perceived varies, and *vice versâ*. This is what has just been proved in the case of touch, and what I am about to show is not the case with sight.

From the special nature of this sense, as contrasted with touch, we might conclude *a priori* that it is the sense peculiarly perceptive of distance. Many philosophers however teach that sight, with the other senses, are modifications of touch. Sir W. Hamilton, upholding this view, states that

the immediate object of vision is the rays in contact with the organ. If he had considered the physical import of the words he would hardly have maintained such a doctrine. Contact is usually understood to mean the approach of two bodies, so that no space intervenes between them. But in this sense there is probably no such thing as contact in nature. Physically speaking, bodies in contact are only at such a distance that there is a sensible resistance to nearer approach. Sensation by contact then is sensation by resistance. It has indeed been proved experimentally that a certain degree of pressure is necessary to produce the sensation of touch.* To say then that sight is a modification of touch is to say that the antecedent of vision is the exercise or feeling of the same repulsive force, which is a physical hypothesis, and, considered as such, is in fact absurd. Between ponderable substances and light contact (in the sense just specified) is either impossible or is the normal condition. The densest bodies admit of light passing through them. On the undulatory theory an undulation only is propagated through an ether which pervades the eye as well as other bodies. In no sense does anything touch the eye. On the emission theory the particles move freely through the retina, which is transparent, and are absorbed, not repelled, by the opaque choroid. Whatever theory is the true one, we certainly do not perceive either the particles or the undulations, and if we did so, the particle or undulation is not the ray, and contact with a ray is unmeaning. Nor is vision produced by a single ray, but by a converging pencil; the pencil however is not the object of vision; what we see is a bright point, external and distant. It is possible, indeed probable, that

* Aubert and Kammler, in Moleschott's "Untersuch.," vol. v., p. 153. The minimum pressure is stated at ten milligr. But in the less sensitive parts, as on the back, &c., heat may be mistaken for touch. See the experiments of Wunderli, ibid. vol. vii., p. 393.

the physical antecedent of the sensation is the same in both sight and touch; heat for instance, or some electrical condition; but if this were proved we might just as well call touch a modification of sight as *vice versâ*. Such a discovery would in fact abolish the specific notion of sensation by contact altogether.

But even if it were allowable to say that the object of vision is perceived by a modification of touch it would by no means follow that it is perceived as in contact with the organ. For it has been already shown that we have no intuitive knowledge of the place of any organ. It is by sight and touch that we learn the position of the external parts of our body. In the case of the eye however there is a further and peculiar difficulty. Sight can plainly give only a vague and negative notion of the place of the seeing organ. Nor can touch discover it to us as it does that of the tactile surface of the body, namely, by compelling us to associate a certain part as felt with a certain organ as feeling. For, in the first place, it is only by rare accident that we touch the eye-ball; the sensitive organ, which is not external, cannot be touched at all; and secondly, touching either the eyelid or the eye (pressure being out of the question) produces no visual sensation. The hand then cannot by contact with any part produce vision: but it can when not in contact interrupt the sensation. It follows that when once we have learned to know our own hand by sight, and it is probably the first organ known, the fact that we see it when it feels no contact would imply a contrast in consciousness between vision and perception by touch. There is another and even more essential reason why the object of vision cannot be seen as in contact with the eye. To perceive an object as in contact with the organ is to refer it to the same place as the sensation through which it is perceived. Now, in vision we are not conscious of any sensation whatever; we have therefore no

knowledge of the organ as sentient. Consequently it is impossible that we should see any object as in contact with the eye before we have acquired the knowledge of its place. If a demonstrative proof of this is sought we have it in the fact, that notwithstanding all our experience we cannot with the utmost attention distinguish the impressions on one retina from those on the other. Even when we intentionally produce double vision it is not by sensation, but by reasoning, that we are able to say which image belongs to either eye.

A striking proof of the correctness of these views is found in the phenomenon of erect vision, the retinal image being inverted. On the supposition that we see objects at first as in contact with the eye, which implies a knowledge of the place of the impression, erect vision as well as single vision is simply inexplicable. But when it is admitted that the local impression is not itself perceived, but is the means of perception of the external object, there is no difficulty to be solved, except in the circumstance that a difficulty has been imagined.*

Since then the object of vision is not felt as in contact with the organism, and yet is perceived as external, it must of necessity have its place in space of three dimensions. This being so, we are prepared for the fact that the special object of sight, and that of sight alone, is inseparable from extension even in thought.† This perception is the only one which of necessity calls for the *a priori* idea of space. To this fact we have an unprejudiced testimony from Arthur Collier. In

* "The only difficulty," says Sir D. Brewster, "which I have ever experienced in studying this subject has been to discover where any difficulty lay." Edinb. Trans., vol. xv., p. 354. There is an ingenious paper on the subject by Dr. Alison in the same Trans., vol. xiii.

† Hamilton, Dissert. on Reid, D. p. 860 *b*, note. Lectures, vol. ii., pp. 165, 167.

the words of this acute idealist, "the seeming or quasi-externeity of visible objects is . . . . a natural and necessary condition of their visibility. I would say, that though God should be supposed to make a world or any one visible object which is granted to be not external, yet by the condition of its being seen it would and must be *quasi external* to the perceptive faculty."* And that he considered this character to be peculiar to the sense of sight seems evident, inasmuch as throughout his entire argument against the externality of the sensible world he treats of it only as it is made known by vision. Feeling he mentions only by the way as suggesting an objection to his conclusion.†

To the testimony of Collier I may add that of an eminent physiologist, Funke. "The fact that closing the eye precludes vision leads us to conclude that the eye is the organ through which we see; but the eye seems to us, as it were, only an opening through which an inner power of seeing presses into the outer world. Nay, strange as it seems, we place in the space without us not only the entoptic appearances in the open eye, but also the subjective appearances in the closed eye, persistent impressions, &c., and this to such a degree, that even when the subjective image of a flame is observed in the closed eye we involuntarily form a judgment of its distance."‡ Similarly in the experiments of Purkyně, in which the arteries of the retina, or the (so-called) moving blood corpuscles, are made visible, these appear external.

But the intuition of space is required, not only by the nature of the sense, but by that of its perceptions. Space is a form of intuition of co-existing objects, and its empirical apprehension cannot be necessitated, and therefore not in the first instance determined, except by the perception of co-ex-

* "Clavis Universalis," in Metaphysical Tracts, Introd., p. 5.

† Pt. ii., ch. 2, obj. 2.

‡ "Physiologie des Menschen," p. 836.

isting objects. Whether perceptions are like or not, if they are present only at different times, there is no reason why they should occasion any idea of difference of place. But where two or more perceptions of the same kind are present at the same time their objects must appear in different places. Now, even as regards superficial extension, sight is the only sense which from the nature of its organ must occasion such an idea. The organ of sight, like that of touch, may be regarded as consisting of a number of separate organs, each sensitive element being distinct from every other. But in the case of sight all these are always active, always sentient, whether a definite image is present or not.* We are therefore not only compelled to see the parts of the external field as locally separated or extended, but the interval between any two points is definitely perceived. In touch, on the contrary, as already remarked, the perception of co-existing objects is only accidental, and even when two points are impressed at once there is no knowledge of the interval between them, in which no sensation exists. As to extension in the third dimension, it is clear that touch does not furnish the necessary condition, since whatever part of a nerve is affected the affection is the same, and the object is referred to the same place as the sensation. Indeed only the extremity can be normally affected. Depth in space then can be perceived by touch only successively, and all such perceptions are already provided with their suitable and sufficient form of intuition in time. We have just seen that in the perceptions of sight the required condition occurs from the total absence of any organic sensation, which compels us to separate the subject perceived from ourselves. But this is not all. When we look from a near object to one a little farther off the

* The perception of black is no exception, being the effect of the activity of the organ. See Volkmann, Art. "Sehen," p. 265. Fechner, "Psychophysik," i., 165.

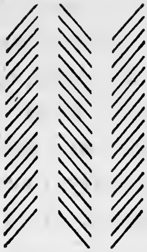
ocular adjustment is accompanied with a certain motion, more suitable indeed than that of the hand to suggest distance, because for the same distance it is always the same, but still only a succession. But the former object continues to be seen, although with decreasing distinctness. Through a certain short interval both objects may be seen distinctly. This co-existence demands the idea of space as the indispensable form of its intuition. But what space? Not the superficial extension already known, the perception of which is accompanied with a totally distinct sensation. Suppose, for example, such a figure as a cube or the letter L placed on the table so that the horizontal line is directed towards us. In looking along the vertical line we are aware of the co-existence of certain distinctly visible parts, and by a certain motion can bring each in succession into the axis of vision. Thus we pass by a continuous change to the foot of the vertical. Now it will be proved presently that the eye requires a varying adjustment, in order to see distinctly at different distances. In moving the eye therefore along the horizontal line we find a new kind of sensation added to the former, and we also find the previously seen parts now continuing to be seen, but with a quite different kind of distinctness from before. If the horizontal line be near and long enough, the parts not directly looked at will be seen double; and this will furnish another peculiar antecedent. Here then first are the conditions for the perception of trinal extension. The facts may be stated in a different form, thus: Even were there no power of adjustment whatever, we find that with respect to some objects we can alter the apparent lateral distance between them by a voluntary effort—viz., a motion of the head or of the eye only; so that of two objects which were at one time in the same visual line, one may be brought to appear either at the right or the left of the other, the change being wholly dependent on our will; while with respect to other objects no such change is possible. In the former case,

we cannot conceive the two objects as occupying the same place although seen in the same visual line; nor yet can they be conceived as situated beside one another in the same plane. The possibility of representing them at all, as co-existing, implies therefore as a condition the representation of distance. This is independent on any power of adjustment. Where this power exists one set of objects can be seen distinctly at one time, while we are cognizant of another series which may be in the same visual lines, but can be brought into distinct vision only in succession by a voluntary effort, and at the expense of the distinctness of the former. In this case again the objects cannot be conceived to be in the same place, although their projected places are identical. When both eyes are used objects out of the limits of single vision are still less distinct, since each eye sees through the image belonging to the other or covers it with a different image. But these again by a voluntary effort may be brought into the field of distinct single vision.

One or two circumstances deserve to be mentioned which act as motives, so to speak, to the appearance of solidity; one is the spherical form of the retina, owing to which the images received on it cannot be projected or represented on a plane without distortion. One result of this is that angles less than a right angle appear generally too large. This may give rise to a discordance between the apparent position of points and lines and the apparent magnitude of the intervals and angles between them. This fact has some important consequences; but without entering on the geometrical discussion of it, it will be sufficient to give an instance of the illusions which appear to be traceable to this cause. Thus the parallel lines in the subjoined figures appear inclined. This illusion is most striking when the paper is looked at obliquely, but so that the long lines are still perpendicular to the line of sight; when we look along them, *i.e.*, when they are but little inclined to the line of sight the

FIG. 1.

FIG. 2.



illusion disappears entirely. Again, in fig. 3 the narrow line appears broken, each portion being turned about a different point.

FIG. 3.

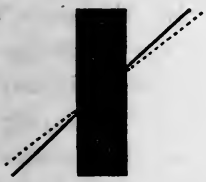
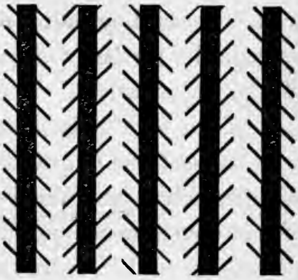


FIG. 4.



In fig. 4 both these effects are combined, and the result is a very striking double illusion.* In both cases there is an in-

* This remarkable illusion was first noticed by Zöllner, Poggendorff's Annals, 1860, vol. ii. A geometrical exposition of these illusions (but imperfect) may be found in Hering, "Beiträge zur Physiologie," vol. i., sec. 26. I have observed the same illusion in the case of a pair of slender spiral columns with opposite twists. This observation and that with the glass plate are opposed to Volkmann's explanation—viz., that the oblique lines suggest the idea of an oblique plane. "Physiol. Untersuch. im Gebiete der Optik." (1863), p. 163. In the case of the transparent plate, as soon as the oblique planes are visibly suggested the long lines recover their parallelism. Indeed the vertical lines are not necessary to the illusion first mentioned (see fig. 2).

G 2

consistency in the appearances presented to the eye, which cannot be removed as long as the figure is seen in a plane. But if these figures are drawn on a piece of glass and held up to the light, there is nothing to interfere with the perception of depth which at once obtrudes itself, and the eye then rests as if satisfied.

And lastly, I may notice the varying magnitude of the retinal image. This could not perhaps of itself suggest the idea of distance from the organ, but that being given it is adequate to suggest its variation. Fortlage indeed assigns a more important place to this change of magnitude. "There is," he says, "in every sense a readiness to project the objects it perceives into the outer world, as soon as any difficulty is found in conceiving them in the place where they appear; and the visual images are projected out of the eye simply because from their unsteadiness they found no place there." "No other sense," he remarks further, "advances us so much in the knowledge of the outer world. The fundamental cognition is indeed that of our own body given by touch and the accompanying active sensations of gravity and resistance; but this remains with its active and passive sensations confined to the limits of our own person, and would hardly come to an elaborate knowledge of the outer world without the help of a sense which as it were takes us out of ourselves."* This conclusion is more than justified by the preceding considerations. Sir W. Hamilton also holds it to be probable that the perception of space and figure belong properly if not exclusively to sight.† Yet while thus contradicting the postulate on which Berkeley's theory is founded, he speaks of the proof of that theory (as far as it concerns the visual measure of distance) as being to all appearance complete. This question of the measure of distance must be now considered.

* "Psychologie," vol. ii., pp. 338, 336. † "Lectures," vol. ii., p. 169.

CHAPTER VII.

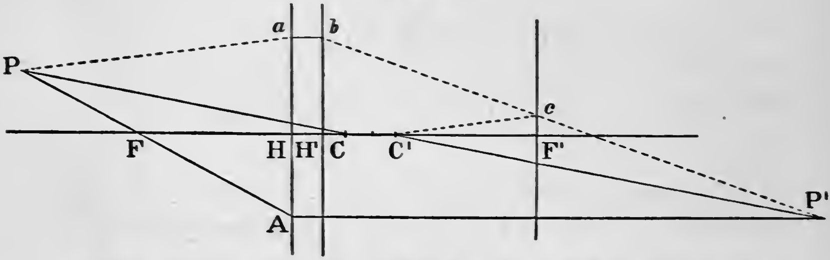
PERCEPTION OF DISTANCE IN VISION WITH ONE EYE.

If it be granted that the perception of extension in three dimensions is proper to the eye, it does not follow that there must be variations of visual sensation corresponding to variations in the distance of the object. We are perfectly conscious of the resistance of the ground although incapable of estimating its amount. It would still therefore be possible that apparent distance should vary as irregularly as apparent heat for instance actually does. It is however matter of experience that the measure of distance even with one eye agrees in general with the external reality; and it is an interesting inquiry, what are the sensitive impressions which correspond to the degrees of the perception? We are not yet in a position to state these with certainty, since even the intimate structure of the eye is only beginning to be known with any exactness. Nor is it necessary for our present purpose that we should do so; it is enough if we can indicate any variety in the impression which does in fact correspond to the variety in distance.

It will be proper, in the first place, to state certain important facts relating to the optical construction of the eye itself, and the course of a pencil of light within it. The refracting surfaces of the eye are not spherical, the cornea being ellipsoidal, and the lens in its anterior surface ellipsoidal, and in its posterior paraboloidal. They may be considered

however as nearly spherical. Now in any system of spherical surfaces having a common axis FF′, we can find the position of the image of a point P by the following simple construction:—F F′ being the two foci, and H H′, C C′, four

Fig. 5.



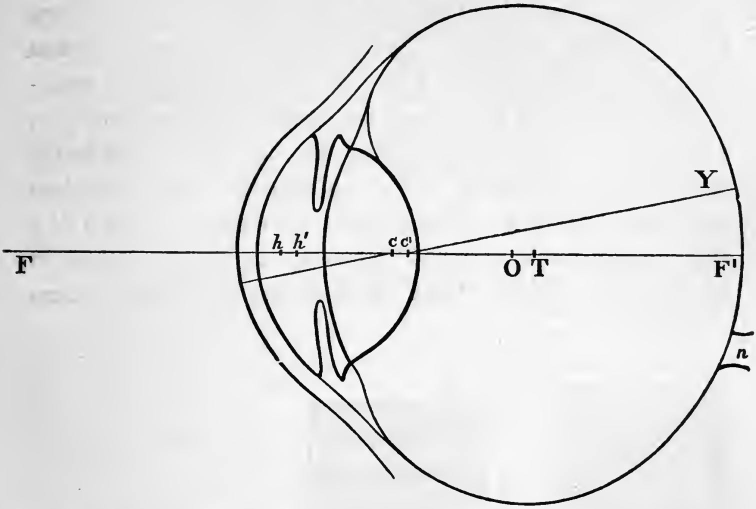
points easily determined, draw PC, and parallel to it C′P′. Draw also PF, and let it meet in A the plane through H perpendicular to F F′. Then let A P′ parallel to F F′ meet C′ P′ in P′; this is the place of the image.* In the eye, the interval between the points C C′ is less than half a millimetre, they may therefore be regarded as coincident, and called the optical centre. The point F′ (fig. 6), where the optical axis meets the retina, is not the most sensitive point. This, which also determines the centre of the field, is in the yellow spot Y, which in normal eyes is considerably to the outer side of F′. The line Y C, being the direction of distinct vision, may be called the visual axis, and it forms an angle with the optic axis of 5° on an average.†

The surfaces of the cornea and the lens are not, however,

* To find the course of any ray P *a* draw C′ *c* parallel to it to meet the focal plane F′, and *a b* parallel to F F′; then *b c* is the final direction. Listing "Dioptrik der Auges," in Wagner's Handwörterb.; [Gauss "Dioptrische Untersuchungen," Abhandlungen der Gött. Gesellsch. Bd. i., 1838–1843].

† *I.e.*, in normal eyes; less in myopic and greater in hypermetropic eyes. Donders "De ligging van het draaipunt van het Oog." Verslagen en Mededeel. der K. Akad. van Wetensch. (Amsterd.), vol. xiv., p. 364.

Fig. 6.



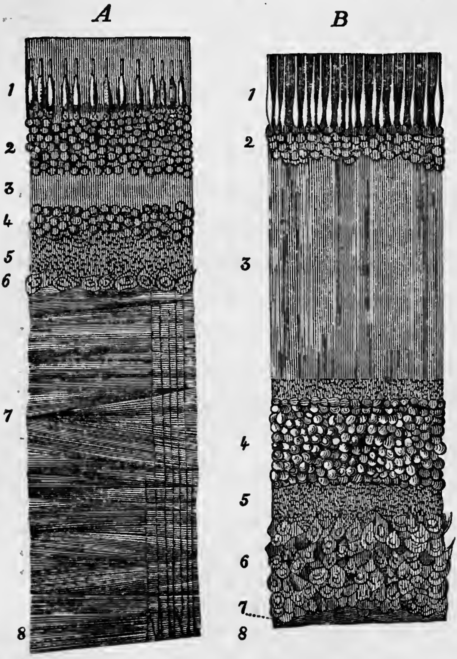
spherical, but ellipsoidal, and therefore the refracted pencil is not collected into a point, but into a thin bundle of some length, or a "focal line."* And further, as these surfaces are not generally symmetrical (or of revolution), they have a different focal length in the vertical and horizontal sections; so that the distances of distinct vision for horizontal and vertical lines are often sensibly different.† The bundle of rays is in fact towards one end broader in the vertical direction, and towards the other in the horizontal.

* See Sturm "Comptes Rend.," vol. xx. (1845), pp. 554, 761, 1238, where the form of the refracted pencil is investigated. It is owing to this that the eye has the appearance of achromatism, the "focal intervals" of rays of different refrangibility being partly superposed.

† This astigmatism (which exists in most, if not all eyes) was first noticed by Young, Phil. Trans., 1801. cf. Plateau, "Bullet. de l'Acad. de Bruxelles," 1834, p. 96. Helmholtz "Physiol. Optik," pp. 140, 145. Fechner's "Centralblatt," 1853, pp. 73, 96, 374, 558. Cornelius, "Theorie des Sehens," p. 301. [Fick, "Medicin. Physik.," p. 328]. Wharton Jones, Proc. R.S., vol. x., p. 381. Procter, Philosoph. Magaz., vol. xxvi. (1863), p. 295.

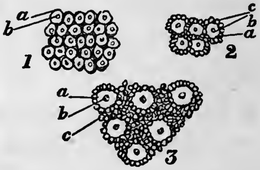
As it is occasionally necessary to refer to the microscopic structure of the retina, it may be convenient to mention here the most essential facts. Light falling on the retina passes through a layer of fibres of the optic nerve extended transversely over it; it then traverses certain layers of granules, and one of vesicular or ganglionic nervous matter, and lastly enters the "bacillar layer," which consists of rods set close and perpendicular to the surface of the retina, arranged like the columns in the Giant's Causeway, or like the florets in the disc of a daisy. Some of these are cylindrical, others

FIG. 7.



Perpendicular section of the retina A, near the entrance of the optic nerve; B in the yellow spot. Layers numbered from without inwards—1, bacillar layer; 2, 3, 4, outer, intermediate, and inner granular layers; 5, finely granular layer; 6, layer of nerve cells; 7, fibres of optic nerve; 8, bounding membrane. After H. Müller.

FIG. 8.



Bacillar layer seen from without—1 at the yellow spot, 2 at the boundary of this spot, 3 at middle of retina; *a* the spaces corresponding to the cones, *b* rods of the cones, the ends of which are often situated deeper than the ends of the proper rods *c*. Mag. 350 times. After Kölliker.

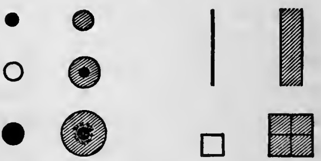
club-shaped; the latter are the "cones or bulbs." The diameter of the cones is about ·0025‴ (lines), that of the rods ·0008‴, the length ·025‴ to ·036‴ according to Kölliker. In the most sensitive spot (the yellow spot) there are no rods, but cones only. Many reasons are given by H. Müller for regarding these, as the ultimate percipient elements.*

* "Anatom. Physiol. Untersuch. über die Retina." Leipz., 1856, p. 97. The most important positive argument in favour of this view is, that the rods are continuous with the granules, and through these with the ganglion cells and nerves. They are in fact the terminations of the optic nerve, from each fibre of which "a thread-like body passes radially through the thickness of the retina, including in its outward passage a vesicle; again, beyond this, a granule, and still farther a cone, and terminating in a rod." (Draper, "Human Physiol.," 2nd ed., p. 391). Hannover and others maintain that this bacillar layer is simply a reflecting apparatus by which the light is concentrated upon the fibres of the optic nerve. In form and structure it is as unsuited for such a purpose as the nerve fibres, which are transparent, are for receiving the impressions. The fibres of the optic nerve are not only most numerous where the retina is insensible, and fewest where it is most sensitive, but they extend transversely and are superposed on one another; so that each pencil would meet many fibres, and each fibre receive many images. The accurate perception of figure and of intervals appears to require a regular arrangement of the sensitive elements, such as the cones and rods alone present. The inner layers are not only irregular, but interrupted by blood-vessels. When, as in Purkynê's experiments, these are made visible they have a sensible parallax; whence it follows that the sensitive surface is at some distance behind.* There is moreover some correspondence between the diameter of the cones and the smallest distinguishable interval. Finally, in the cephalopoda, this layer corresponding to this is the only one that is not behind the dark pigment, and therefore the only one reached by the light at all. It is therefore highly probable that the rods and cones are the primary sentient elements. There is, however, a fatal objection to the view that these bodies receive the impression of light immediately. Light, to be perceived, must produce some change in the organ; now, by the principle of conservation of force, in being thus transformed into an organic impression it ceases to be light; in other words, the same force cannot be expended twice over—first, in causing sensation, and again in propagating an undulation. But the rods and cones are transparent.

* Helmholtz "Physiolog. Optik," p. 161.

Now that the eye requires, and generally possesses, a power of adjustment or accommodation in order to see distinctly objects at different distances is a fact long familiarly known, although not alluded to by Mr. Mansel or Mr. Mill. It may be readily verified by looking through a pane of glass at a more distant object, when a small mark on the glass will appear confused and enlarged; while by a voluntary muscular effort, directing our gaze to the latter, we see it distinctly, and the remote object now confused. With a little practice, it is easy to alter the adjustment at will while looking at the same object, as for instance, a mark on the paper or on glass. In particular, I find if I adjust the eye for distant vision, while looking at a black dot on the paper, the dot becomes a circle with grayish centre; a small circle appears larger, but with a black dot in the centre; a larger spot appears as a dark nucleus surrounded with a penumbra; a straight line appears double, the middle space being gray; and a square, by suitable adjustment, will appear larger, but with a cross in the middle. These appearances may be gradually

FIG. 9.

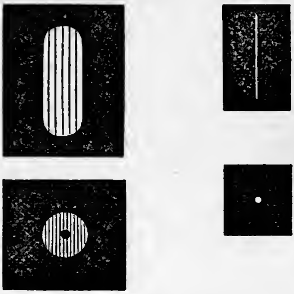


Besides it is plain that the impression of light ought to be received on a definite surface. Now the rods terminate in such a definite surface in contact with a dark pigment. Here first the light ceases to be transmitted as light. It has been supposed therefore by Draper, that the absorption of light in this pigment develops heat, and that the rods conduct the impression to the nerve cells, which constitute a ganglion capable of perceiving light, the fibres of the optic nerve merely communicating the impression to the sensorium.* On an entoptic observation suggested by Purkynê, in which the distribution of the rods and cones appears to be rendered sensible, see Czermak, in the Vienna "Sitzungsberichte," vol. xli., p. 644; xliii., pt. ii., p. 167.

* Draper, op. cit., p. 387. According to Dr. G. Wilson, however, the total absence of pigment from the choroid is compatible with perfect vision, as in the case of albinos. Edin. Trans., vol. xxi.

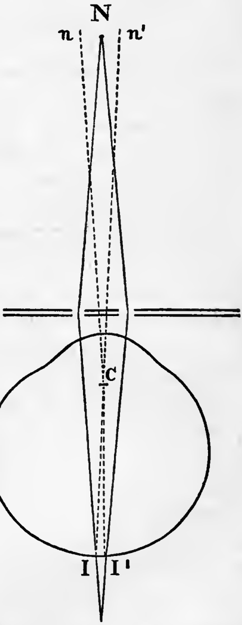
altered at will, and they are obviously the result of imperfect focussing. They are represented in fig. 9.

FIG. 10.



The effect is better observed by making a very narrow slit in a card, and holding it up to the light. Looking at the slit itself it appears distinct and narrow, but if we look through it at the window or the lamp, the slit appears much wider and divided by a number of parallel lines. Looking similarly through a pinhole it will appear enlarged with a dark spot in the centre. These appearances are shown in the diagrams at the left side of fig. 10. Another experiment of a similar kind is that of Scheiner. Make two or more pinholes in a card in a space less than the aperture of the pupil, and, holding the card close to the eye, look at a needle six or eight inches beyond. It will appear single, but if we look at a more distant object the needle will be seen double, treble, &c., according to the number of holes; the circle of diffusion being broken up into so many separate points (fig. 11).

FIG. 11.



The dotted lines are the apparent directions of the two images.

There have been various hypotheses as to the changes in the refracting media of the eye, by which this adjustment is effected. It has been accounted for by supposing a

change in the convexity of the cornea, in the diameter of the eye-ball, or in the position of the lens. Young showed that there is no change in the cornea; and more recent and accurate observations have shown that there is a change in the form of the lens, the convexity of both its surfaces being increased in near vision, so that the anterior surface approaches nearer to the cornea, while the summit of the posterior surface remains (nearly) in its place. The method adopted in the investigation was, to observe the images of a flame formed by reflection at the two surfaces of the lens and at the cornea respectively. It was found that the middle image diminished when a near object was looked at, indicating an increase in the curvature of the anterior surface. At the same time, it also approached the image formed by the cornea, in consequence of the advance of the summit of the lens.* These

* The change in the form of the lens was first proved by Young, Philos. Trans., 1801; but his method was not very accurate. The observations referred to in the text were first made by Cramer and Donders, in Holland, in 1851 [Cramer, "Het Accommodatie-Vermogen der Oogen," Harlem, 1853; in German by Doden]. See Schauenburg, "Das Accommodations-vermögen der Augen nach Dr. A. Cramer und Prof. F. C. Donders," Lahr, 1854. Nearly at the same time Helmholtz independently made the same observations. Berlin Monatsberichte, 1853, p. 137. Græfe's "Archiv für Ophthalmologie," vol. i., pt. 2. "Physiologische Optik," p. 110. See also J. D. Forbes, Edinb. Trans., vol. xvi. (1849), p. 1. The history of opinions on the subject is given by Helmholtz, "Physiol. Opt.," p. 118. On the muscular mechanism by which the adjustment is effected, see Henke, in Græfe's "Archiv," vol. vi., pt. 2, also H. Müller, ibid., vol. iii., pt. 1; Mannhardt, vol. iv., pt. 1; Czermak, vol. vii., pt. 1; ("Ueber das Accommodations-phosphen,") and Langenbeck, "Annales d'Oculistique," vol. xxiv. The most probable hypothesis appears to be that in the position of rest the lens is flattened between the leaves of the zonula Zinnii, and in near adjustment the ciliary muscles or tensor chorioideæ contracts, causing the zonula to relax and permitting the lens to assume its form of equilibrium, which is the more convex. If the tensor suddenly ceases to act, the tension of the zonula causes a tug on the retina; and hence Czermak accounts for the luminous ring which

changes being measured, the corresponding alteration in the form of the lens could be determined with considerable exactness and the limits of distinct vision deduced, as far as they depend on this cause. These agree so closely with the actual limits as observed in several persons, that the sufficiency of the cause may be regarded as proved.* It is further asserted that where the lens is absent there is not the least trace of any power of adjustment.† There is, however, reason to suppose that there may be also a slight displacement of the retina itself.

It is not necessary for distinct vision that the rays from the point looked at should be brought exactly to a point in the retina, which we have seen indeed is not generally the case. As long as they are contained within the area of one sensitive minimum, the sensation will be that corresponding to the vision of a point. A certain amount of dispersion then does not interfere with distinct vision; and accordingly it is easily shown by actual experiment that we can see simultaneously and distinctly objects at slightly different distances. The interval is called by Czermak the Accommodation-line; and by a somewhat rough method he found that for his own eye the accommodation-line commencing at a distance of seven inches reached to ten (at least), and that commencing at thirteen inches extended to nineteen.‡ The greater the

is seen when in the dark the eyes, having been voluntarily adjusted for near vision, are suddenly allowed to assume the position of rest.

* Knapp, in Græfe's "Archiv," vol. vi., pt. 2, pp. 33, 37. Combining Knapp's results with those of Helmholtz, we have on an average of six cases the radius of curvature of the anterior surface of the lens in near vision nearly 4 lines, and in distant vision 2·6, and of the posterior surface 2·8 and 2·3 respectively.

† Young, l.c., p. 64; Donders, in Græfe's "Archiv," vol. vii., pt. 1, p. 155, 168; Knapp, l.c., p. 51; Sir E. Home's observations to the contrary effect were made a year after the operation for cataract had been performed, so that the lens might possibly have been regenerated. See *infra*.

‡ "Physiolog. Studien" Sitzungsberichte der k.k. Akad zu Wien, vol. xii., p. 322. sqq.

distance from the eye, the longer the accommodation-line, until at a certain limit it becomes infinite; the adjustment of of the eye for parallel rays being sufficient for the distinct vision of all objects beyond this limit. Czermak fixes this at sixty-five metres (about seventy-two yards), but the grounds on which he does so lead us rather to a much lower estimate—viz., about thirty yards. This may be called the limit of adjustment. This of course varies considerably in different persons.*

* When the eye is adjusted accurately for remote vision, rays from a point 65m. distant are diffused over a circle of which the diameter is ·0011 millim., or ·000043 inch=·0005‴. But as the diameter of a cone or sensitive element in the yellow spot is only ·0025‴ to ·003‴, this dispersion, says Czermak, is decidedly within the necessary limit. True; but how far within it, is the question. Czermak appears to have fixed on 65m. for the sole reason that it stands at the head of Listing's table of focal aberration and circles of dispersion (Art., "Dioptrik des Auges," in Wagner's "Handwörterb. der Physiol.") Listing himself remarks that at 25m. or even 12m. the dispersion is generally insensible (p. 500). If we compare the arrangement of the cones in the yellow spot to that of a series of hexagons or of circles in juxtaposition, and consider the images of a pair of parallel lines falling on them, it will readily appear that a dispersion extending to one-third of the diameter of a cone can hardly affect distinctness of vision even at small distances. But this would give thirty-two yards for the limit above mentioned. In the observations on the accommodation-line just referred to, the dispersion neglected amounts in the first case to nearly ·044‴, and in the second to the ·025‴. Reid mentions fifteen or sixteen feet as the limit of adjustment ("Works," p. 189). In this he probably followed Dr. Jurin (in his paper at the end of Smith's Optics), whose calculation was based on the assumption that the least angular distance at which two stars can be recognised as distinct is about 4′. He reasoned that for less intervals the diffusion circles must overlap, and this would be the case if the eye in looking at distant objects were adjusted to bring to a focus on the retina rays from a distance of twelve or thirteen feet. Hooke, with more correctness, estimated the least visible distance between two stars at $\frac{1}{2}$′. He observes, however, that two stars less than 1′ apart are seen as one by hundreds. Very few can distinguish an interval of $\frac{1}{3}$′ ("Posthumous Works," (1705), pp. 97, 98). I find that remote objects begin to appear indistinct when I look at an object within twenty-three yards. This would give for the circle of diffusion ·0014‴, or about half the diameter of a cone. This is nearly what Bergmann found

That we have even with one eye a tolerably correct perception of distance, which cannot be compared with the impression produced by a picture, is evident, even from the consideration of the stereoscope. For while the effect of the combination of the two pictures is so strikingly different from the appearance of a single picture, it must be confessed that there is not such a difference in the appearance of the natural objects themselves as seen by one and by both eyes. The perception of relief in the two cases differs in degree only, not in kind. Now when one eye only is used, our perception of distance and magnitude is dependent in great part on the adjustment employed. Objects distinctly seen appear nearer in proportion as the effort of adjustment required is greater; those which are sensibly indistinct are generally referred to a distance less determinate, but depending on the amount of indistinctness and other circumstances. Thus, if we are looking through a window at a distant view, a fly on the glass is mistaken for a bird at a distance. The image on the retina is only an indistinct dark spot, of a magnitude which would correspond equally well with either, but too indistinct to suggest its own distance. The eye, however, being adjusted for distant vision, the dark spot conveys the idea of a distant object. Again, if we hold a pencil between the eye and a window twenty or thirty feet distant we shall observe that when we look at the pencil the window will

by experiment to be the average minimum sensible interval. His observations were made with a series of parallel lines one millim. broad, and at intervals of one millim.; and he found that in order that the direction of the lines should be perceived the retinal interval should be that now mentioned. The observations of H. Müller and of Volkmann give a similar result. Cf. Funke, "Physiologie," p. 852; Cornelius, "Theorie des Sehens," pp. 340, 341; [Bergmann, in Henle and Pfeuffer's Zeitschrift, vol. i., p. 83, 94]; H. Müller, "Quarterly Journal of Microscop. Science," vol. i.; Volkmann, Art "Sehen," in Wagner's Handwb., p. 331. It may be remarked that the chromatic dispersion of the eye amounts to ·019‴ (Helmholtz, "Physiol. Opt.," p. 331).

appear smaller, and when looking at the window the pencil will seem larger.* Now in both cases the known distance is the same, and in the former the image on the retina is even enlarged slightly by diffusion; the diminution of the apparent magnitude must therefore be owing to the adjustment. The phenomenon noticed by Wheatstone and Brewster that in figures composed of red and blue lines, the red appears raised above the blue even to one eye, is sufficiently accounted for by the latter from the same principle; since the eye not being achromatic, pencils of different colours require a different adjustment.†

Now this perception is not occasioned directly by the muscular sensation of adjustment either changing or fixed; for when the eyes are closed we can adjust them at will, but no idea of distance is suggested. It must therefore be the result of a visual sensation accompanying the adjustment. It will not be out of place to consider what this sensation may be; but it must be borne in mind, that for the purposes of the present argument it is enough to show the possibility of such a sensation.

When we view at once a near and a remote object one of the two images must be indistinct or diffused. That this diffusion has some influence on the perception of distance appears from observation, of which one just given is an example. But its effect is very different, according to the part of the retina on which it occurs. This requires some explanation. The sensibility of the retina decreases very rapidly from the central spot towards the margin; so that at 10° from the axis the smallest visible line is at least two and a half times as broad as in the axis. The power of sepa-

* Ludwig "Lehrbuch der Physiol.," vol. i., p. 252. Panum in Græfe's "Archiv," vol. v., pt. i., p. 15.

† Brewster, Report of Brit. Assoc., 1848, p. 48. W. White Cooper, in Todd's Cyclop. Art. Vision, p. 1450.

rating lines decreases much faster; so that in Volkmann's case the minimum interval sensible at 8° from the centre (inwards) was one hundred times greater than in the centre.* In other words, the resolving power was a hundred times less. Upwards and downwards the decrease is still more rapid. In consequence of this diminished resolving power the lateral parts do not require so delicate an adjustment as the centre, and at a certain distance from the axis the distinctness of the field is constant, whatever be the adjustment employed. But we must not confound the vagueness of imperfect sensibility with the indistinctness or diffusion of imperfect focussing. In the former case, although a smaller number of sensitive points is affected, the apparent magnitude of the object is not diminished in proportion; the outline is filled up therefore vaguely, perhaps by a kind of irradiation, and the result is a perception so indefinite that we could not pretend to copy accurately on paper the appearance of an object seen much out of the axis of vision. But when a diffused image is formed on the central spot the perception is still definite, so that we can readily copy the appearance of indistinctness. Now, if the appearance of a black line on paper under these conditions be observed, it will be found that the effect of the diffusion is to make it double, or rather multiple;† the intervening space being partially shaded. A

* Art. Sehen, in Wagner's Handwb., pp. 332–334. The most complete observations are those of Aubert and Förster, in Græfe's "Archiv," vol. iii., pt. ii., p. 1. Poggendorff's Ann., vol. cxv., p. 87, 116, 249. The most obvious account of these facts is to refer them to optical causes. The optical centre of the eye being considerably before its geometrical centre, all lateral images must, one would suppose, be confused. But the contrary appears from the observations of Aubert and Förster on the eyes of animals, op. cit., p. 34. Nor does the appearance of objects, *e.g.*, of two black squares, seen indirectly, correspond with that caused by diffusion. It may be added, that if this were the cause a convex lens would make the image distinct, which is not the fact.

† These phenomena are so analogous, or rather identical with those of

luminous line appears as shown in p. 91, and a dark edge seen against a luminous background shows similarly a series of dark fringes. Now, it is only near the centre that these lines can be discerned; at a little distance from it they produce a single impression. But it is otherwise with the perception of depth. If, while looking at a book or other object a yard or two distant, we hold a pencil between it and the eye, we can perceive the diffusion of the outline, but cannot estimate accurately the distance of the pencil from the book. In order to be able to do this we must move it towards the side until the focal diffusion is no longer discernible. It is then in the best position for comparing its distance with that of the book. The reader will not fail to remark how completely opposed this phenomenon is to the theory of association; but it perfectly accords with the analogy of all direct perceptions. For example, the pitch of a sound depends on the number of vibrations which reach the ear in a second; but as soon as we can perceive the vibrations separately they cease to produce the impression of tone. And as long as a pair of stereoscopic pictures is distinguished as two they do not produce the effect of solidity. Similarly in the present instance, in order that the diffused or multiple image should occasion a perception of distance, it must be perceived as one.*

interference, that we are justified in referring them to that principle. Compare p. 90. They are, doubtless, different in different persons. See Procter, Philos. Magaz., vol. xxvi., p. 301. I state the appearances to my own eyes.

* There would be a difficulty however if there were only one set of sensitive elements—the cones. The diffusion of a pencil of light seems to produce no effect as long as it is contained within the limits of one such element. But there is, as we have seen, another set of elements—namely, the rods. These are supposed by Weber, Aubert and Förster, &c., not to contribute to the perception of space; but they are so analogous in every respect to the former that it seems impossible to deny them an analogous percipient function. Their number however decreases towards the centre,

There are thus, in short, produced in one eye two or more slightly different pictures of the external field, which, like stereoscopic pictures (although in a different manner), produce the effect of one. This is illustrated by what Brewster and Claudet have shown, that the picture formed on the glass of a camera lucida is really multiple, and on this account, when viewed with both eyes, presents the appearance of solidity.

The distance thus measured or perceived is not the difference of distance from the eye as centre, but the projection of this on the line of sight, *i.e.*, the difference of distance from a plane through the eye perpendicular to the line of sight. This is perhaps connected with the fact already noticed, that the optical centre of the eye is close to the lens, or within it (see fig. 6), and is consequently more than a third nearer to the extreme parts of the retina than to the sensitive spot. Hence all pencils of rays which strike these lateral parts must apparently be focussed behind them, and therefore diffused on the retina, as if they came from very near objects. Accordingly, lateral objects supply a sort of base from which distances are measured; this base including, as its most important part, the body itself, whether actually seen or not. In such estimation of distance it is clearly of some consequence whether the loss of distinctness in the parts near the centre of the field is slow or rapid. Now, it has been recently discovered that the field of distinct vision is contracted almost in proportion to the distance of the object looked at when that is not too great.*

and in the sensitive spot there are none (see fig. 8); while elsewhere, although their diameter is less than that of the cones, their number contributes nothing to the power of discriminating form. But these lateral parts have, as just noticed, a function of a different kind. May we conjecture that the rods are concerned in receiving the sensation of the diffusion or multiplicity of the image which the cones perceive as one, and thus furnish the occasion for the perception of relative distance?

* Aubert and Förster, in Græfe's Archiv, vol. ii., p. 11. Poggendorff's

The influence of this in the perception of distance may be illustrated by the effect of looking through a conical tube, which seems longer or shorter according as the eye is applied to the wider or the narrower end. It may also suggest a possible account of the phenomenon of the constancy of apparent magnitude referred to in page 57. While the angular magnitude of an object diminishes its proportion to the distant field remains the same.

In order to compare the distance between two objects without moving the eye, the farther of the two must in general be looked at. The reason of this seems to be as follows:—Suppose we place the same object at various distances in the same direction while the eye is adjusted for distant vision; the diffusion or indistinctness of its outline will vary (inversely as its distance, *i.e.*) in the same proportion as its angular magnitude. But this being so, its absolute distinctness is in fact unaltered, except indeed the object is so small that the diffusion bears a considerable proportion to its diameter. When the adjustment is not that for parallel rays the diffusion increases faster than the angular magnitude. It is still however true, in general, that objects at this side of the point looked at have a pretty uniform distinctness, while the images of more distant objects become smaller and more indistinct at the same time.* But further, it has long been known (since the publication of Scheiner's "Oculus," in 1619) that the pupil contracts when near objects

Annals, vol. cxv., p. 93. It has been suggested by Aubert that this may be owing to a slight displacement forwards of the central part of the retina, which there are other reasons for supposing. Poggendorff's Ann., l.c. Moleschott's Untersuch., vol. iv., p. 33. Cf. Czermak, *ibid.*, vol. v., p. 137, and Græfe's "Archiv," vol. vii., pt. i., p. 148. In connexion with this hypothesis, the peculiar structure of the choroid in birds is interesting. See Leydig. "Histologie," p. 236.

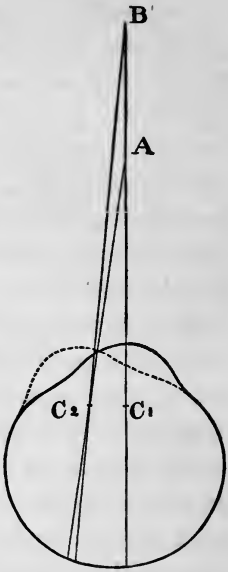
* The constancy of the apparent magnitude will however occasion an apparent diminution of distinctness in proportion to the decrease of the retinal image.

are looked at.* Consequently the light received from more remote objects is proportionately less, and with it their visibility. On the other hand, when a remote object is viewed an increased amount of light is received from those which are nearer. The brightness of the background therefore is less the nearer the object looked at, while the feebleness of the impression of the foreground caused by its indistinctness is counterbalanced by its increased brightness. Hence, when a near object is viewed the background is indistinct and faint, and the distance of its parts is not definitely perceived, but on the contrary nearer objects are comparatively clear, and their distance is definitely perceived. Hence it is that in drawings apparent remoteness may be simulated, not apparent nearness. An object may be made to appear when looked at nearer than the apparently distant background, but when the eye is fixed on the latter the illusion disappears. Sometimes indeed a figure is said to "stand out from the canvas," but it will readily be seen that such cases are only apparent exceptions, and serve to show that the fact stated has the support of universal testimony. It may be added that no pictorial art appears able to produce the illusion of distance even in a slight degree when the picture is viewed close. That this is not owing to the knowledge we have of the painted surface by touch is evident from the case of objects viewed in a mirror, in which on the contrary the illusion is irresistible.†

* The pupil also contracts in a strong light. These two laws may be connected through the enlargement of the field above mentioned. The retina is less strongly impressed by diffused light than by what is collected to a focus upon it; probably because the former is reflected in larger proportion. Consequently when the field of distinct vision is enlarged the effect is the same as if the light were increased.

† At the same time the facts above noticed suggest the remark that a picture may be not the less true to nature, but the contrary, because the foreground, which is not intended to be looked at, is drawn with some degree of indistinctness.

Fig. 12.



It is even possible, as desiderated by Mr. Mill, to see an interval between two objects in the same visual line. If we place a candle so as to be just hidden behind the edge of a screen when viewed directly, then on looking away from it it will become visible. If two objects are placed thus, right and left, then each will disappear when looked at, and reappear when we look at the other.* The bridge of the nose will serve as a screen on one side. The cause of this phenomenon is that the turning point of the eye (T, fig. 6) and the point of crossing of the lines of visible direction (C_1, C_2) are not the same (fig. 12).

The preceding considerations are independent on the motion of the eye. But in practice when we wish to compare two objects, we bring them successively into distinct view by a suitable change in the adjustment. This motion gives rise to a series of changing visual as well as muscular sensations, which may be understood from what precedes, and which is adapted not only to convey the idea of distance, but to give a knowledge of its amount. For it is analogous to the change of sensations which occurs in passing from one object to another in the same plane, although distinct from it, and not accompanied with a change of direction.

Herbart indeed affirms positively that "the eye at rest does not see space. This," he adds, "is difficult to recognise

* Cf. Brewster, Edinb. Trans., vol. xv., p. 351. Even without moving the eyeball the same effect may be produced, though in a less degree, by simply changing the adjustment. The dotted line represents the new position of the cornea.

experimentally, since space being so familiar easily slips into our perception. But if we try to gaze fixedly before us, we shall observe that space disappears, and that in endeavouring to recover it we can detect ourselves in a scarcely perceptible movement of the eye."* But that there is a perception of depth even when the eye is at rest appears from the following observation. If we make a small spot with ink on a pane of glass, and look at it with the eye adjusted for distant vision, it will, as already mentioned, appear surrounded with a penumbra, but the central nucleus will seem sunk within this penumbra, as if seen at the end of a minute tube. And when we look through the wrong end of a telescope there is a very decided appearance of remoteness although the eye is at rest. It is true that in order to measure distance it is necessary to look successively from one object to another, and for reasons already mentioned this motion is also fitted to evoke the idea of distance; but to obtain the impression or perception of distance, it is sufficient to see at once objects at different distances.

With respect to the measure of distance as distinguished from the positive perception, it might be supposed that the medium is supplied by the muscular sensations accompanying the motions of adjustment, &c.; but this view appears to be untenable. The voluntary muscles generally are provided with nerves of sensation, so that when we will a movement we learn by means of these whether the muscles have answered to our effort. But the muscles of the eye seem nearly devoid of sensory nerves.† The reason is obvious; the motions of these muscles are to be guided by the impressions in the retina, not by any sensation of their own position; and it is by the same impressions we learn that our effort has succeeded. It is nevertheless supposable that the

* "Psychologie als Wissenschaft," sec. 111, p. 128.

† See Alison, in Edinb. Trans., vol. xv., p. 62.

voluntary effort may be the occasion of the perception. It is true, when the eyes are closed there is no perception of distance, notwithstanding the effort; but this is to be expected, since the effort is not felt at all as a muscular but as a visual effort, the "looking at" an object, satisfied by distinctness of the image. When no visible object is present, therefore, it cannot convey any perception, just as the raising of the empty hand can give no perception of weight.

It is important to consider how the changes in the adjustment of the eye are induced. As they are to a considerable extent under our control, it might be supposed that they are effected by direct volition. This, however, is not the case. We can indeed change the adjustment, but only indirectly, by directing our gaze to an imaginary object. Of the degree of adjustment itself we are as unconscious as of the contraction or dilatation of the pupil; and although we are no doubt conscious of the effort, our knowledge of its amount is very vague. The effort, moreover, of looking at a vacant point in space is difficult and fatiguing; nor can such an adjustment be retained with any steadiness. On the contrary, when an object is distinctly seen, the eyes retain the appropriate adjustment, not only without effort, but with some positive force.

We conclude, then, for these reasons that the action of adjustment is a reflex action, like the contraction of the pupil in a strong light, or like the motions of a somnambulist, guided by the unperceived impressions of the senses. It is only so far voluntary as it is dependent on our attention to the object. Our looking at it is voluntary, and is followed involuntarily by the appropriate adjustment. The motion belongs therefore to the class of "consensual" actions.* In the next chapter we shall find a further confirmation of this.

If the foregoing remarks were offered as a complete account of the mode in which we perceive distance, it is readily

* Carpenter, "Human Physiology," p. 509, ff.

admitted that they would be unsatisfactory. But, as already observed, we are not concerned to show what the particular antecedent of the perception is, but only to prove its possibility. We are now in a position to demonstrate the existence of a sensation which is adequate and appropriate to the perception. For this it is only necessary to refer to the phenomena of adjustment, and of the accompanying convergence of the visual axes. We have seen (from its effects) that there must be a sensorial or organic affection sufficiently powerful and distinct to determine the appropriate adaptation of the eyes with such quickness and accuracy that in normal vision we are never aware of the double image, or of even a momentary failure to obtain the right focus. It is not necessary for our purpose to know what this affection is. Whatever it is, since it produces a physical effect exactly proportionate to the distance of the object viewed, it is physically sufficient to produce a corresponding perception. It is moreover a visual sensation, and is not confined to objects distinctly perceived.

The foregoing conclusions are confirmed in general by those of Fortlage, who, having given an account of the muscular sensations which accompany the adjustments of the eye, proceeds:—"It is therefore an erroneous view of the matter when it is thought that in the act of sight the addition of the third dimension to the two of the retinal picture, or the projection of this picture into the outer world takes place by way of reflection and judgment. Rather, the degree of distance of objects from the eye is just as immediately sensibly perceived as the degree of distance laterally, only not as the latter in a dynamical way, but by means of effort; they are not measured but weighed, just as height and depth of tone are weighed by delicate muscular contraction."*

* Fortlage "Psychologie," vol. ii., p. 337; Scheidler, Art. "Gesichtsinn," in Ersch and Gruber's Encycl., p. 193.

CHAPTER VIII.

OF BINOCULAR VISION.

ALTHOUGH we have some perception of distance with one eye it is comparatively imperfect; and we must now proceed to consider the special phenomena of binocular vision. That it should be possible at least to see singly with both eyes, is a necessary consequence of the laws of sight, as they have now been stated. For since each retina presents to us an extended field, every part being at some distance, the two fields must admit of superposition, and from their position and angular magnitude (about 150° each) they must of necessity overlie one another. It only remains then to ascertain what are the conditions of their superposition. These will be found to result from the laws of arrangement of each field separately. It is very commonly stated (after Reid, Brewster, and others) that an impression on any point of the retina determines vision in a direction perpendicular to the retina at that point, or, as it ought to be stated, in a line through the optical centre. It is generally added that single vision is the necessary consequence of seeing an object in two intersecting directions. But every object is seen in two intersecting directions; therefore double vision ought to be impossible. We must suppose then, in addition, that each eye gives an accurate perception of the distance as well as the direction. But in that case the slightest error would occasion double vision, whereas we are liable to considerable errors, which however never interfere with single vision. Indeed, it seems absurd to suppose that we construct the place of an

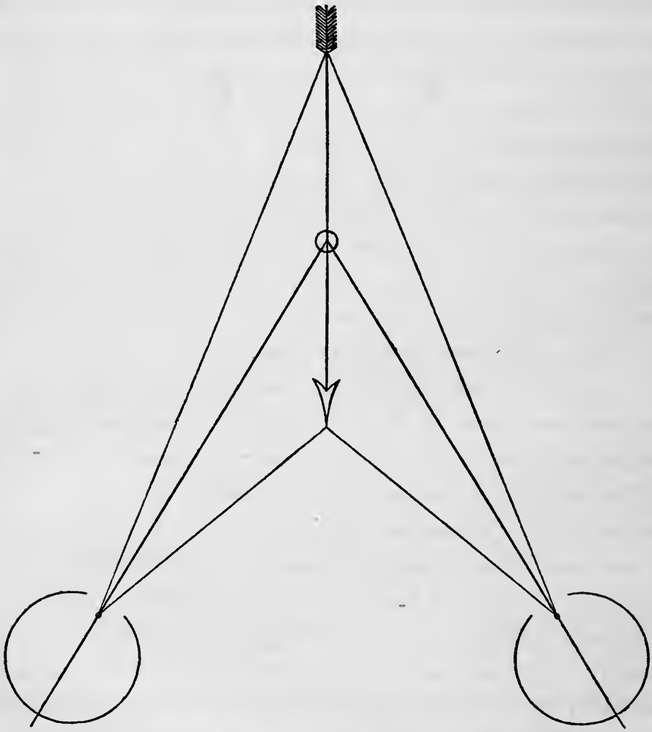
object in such a manner, since, in the first place, we are not aware of the double impression, and, in the second, we do not infer that an object is in such a place, because we see it in two certain directions; but we see it in one direction because it appears to be in a certain place. Any error as to its distance does not affect this one visible direction. Nor can it be said that objects directly looked at are seen in the intersection of the visual axes. Even after experience we have a very imperfect knowledge of the place of this intersection, as may be observed by fixing the eyes on a vacant point in space, and placing an object on what we suppose to be the point of intersection, when the error will be proved considerable. Again, if we unite the two dots below by a proper

convergence either before or behind the paper, the resulting dot will still appear on the paper, and therefore not in the direction of either axis. And if, with the axis parallel, we look through two parallel tubes (of equal aperture), instead of seeing two apertures in two parallel directions, we shall see but one, and that at the same apparent distance as with one eye. If, again, two dissimilar objects are placed before the two openings, they will appear to occupy the same place; yet not only does the law of projection require that they should appear separate, but all the conditions of experience and conception are opposed to the confusion. It follows, that with two eyes we see in one direction. The truth seems to be that the apparent direction of an object is determined by its position in the field, and the centre of the field is determined by the sensitive spot.* This centre appears always directly before us, and the place of all other points is determined by the position of their images on the retina relatively

* See A. Prévost, in "Archives du Bibl. Univ. de Genève," 1859, p. 109. Hering "Beiträge zur Physiol.," p. 132, ff.

to the centre. The law of combination of the two fields corresponds with this view. The two retinas, in fact, comport themselves as identical, point for point. If we call those points "identical" which have, so to speak, the same latitude and longitude on the two retinas, being similarly situated with respect to the middle points,* then: 1°. Impressions on two identical points are referred to one and the same direction, and if for any reason the apparent distance of the objects is the same, they are also referred to the same place. The point looked at, which we may call the "centre

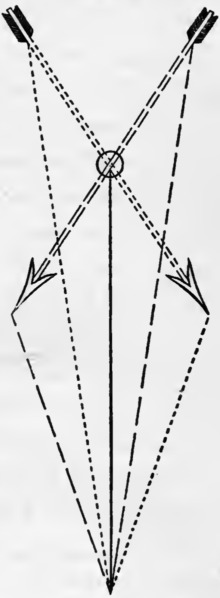
FIG. 13.



* This definition of identical points is convenient, but not perfectly exact.

of the field," obviously fulfils these conditions, as do all the points in a certain straight line through it—namely, that in which planes through the vertical medial lines of the two retinas intersect.* 2° The direction of reference is given by the following construction. Conceive each retina with its field (fig. 13) rotated inwards about this line as an axis by half the angle between the visual axes, so that the two retinas are brought to coincide midway between the eyes, in which position the points which coincide will clearly be "identical points." The point in which the two centres coincide may be called the "binocular centre." Now, the resulting composite field is in all respects the field of binocular vision (fig. 14);† and the direction of objects seen single, as well as of double images, is estimated from the binocular centre.

FIG. 14.



It follows, that with two eyes we see in one direction; and common experience shows that in the case of the object looked at this is estimated from the middle point, as above stated. This once established, the rest of the construction is a consequence of the law of identity. From it we see at once that the true horopter or locus of points seen single is limited, strictly speaking, to the vertical straight line just mentioned. But all points on Müller's horopter (the circle

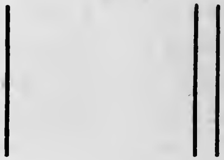
* This line is not generally exactly vertical, but is inclined to the plane of the visual axes at an angle which varies with the inclination of the head and with individual habit. It is easily found in any particular case by forming a double image of a line drawn on paper, and moving the head or the paper until the two images just before coinciding are parallel.

† Hering "Beiträge zur Physiologie," pp. 40, 167. The dotted lines belong to the field of the left eye, the broken lines to that of the right.

through the centres of the two corneas and the centre of the field), are also imaged on identical points, and are therefore seen in one direction; being however at different distances from the two eyes, the images are not identical, and may be referred to different distances.* But in nature this never happens; and the horopter may be said generally to consist of these two lines. They are of course not mathematical lines, but have a breadth and depth proportional to their distance; and when the visual axes are parallel, the horopter includes all points beyond the limit of sensible convergence. The identity of the two fields of vision appears remarkably when we place the two halves of a circle in such a manner, that with the visual axes parallel each eye is directed to the centre of its semicircle. The result is a perception of a complete unbroken circle.

The question now arises, when identical portions of the two retinas receive dissimilar impressions, what is the result? First, let one image consist of two parallel lines, and the other of a single line parallel to the former. On combining the single line stereoscopically with either one of the pair, we perceive in the binocular field two lines,

Fig. 15.



which are no longer in the same level, the outer one seeming nearer, *i.e.*, if the two lines are seen by the left eye, the left-hand one.† If instead of right lines we employ a pair of

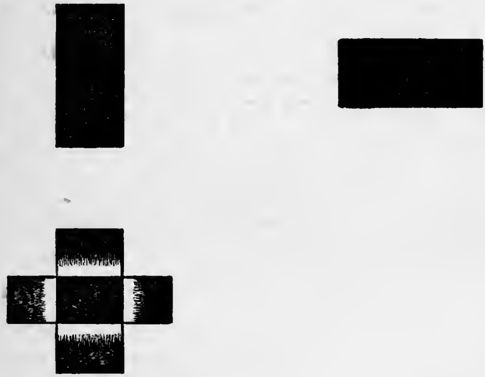
* For a thorough investigation of the form of the horopter in all cases see Hering "Beiträge." The statement above holds in all ordinary positions of the eyes. The fallacies of Meissner's observations which have been adopted in many text-books, are exposed by Hering, Nagel ("Das Sehen mit zwei Augen," Leipz. 1861), and Claparède (Reichert und Du Bois' Archiv, 1859, p. 387).

† When scientific accuracy is sought in such observations as these, it is better to use no instrument; but it is not desirable to repeat experiments in this way too often, as the effort is fatiguing, and probably injurious to the eyes.

FIG. 16.

concentric circles on one side, and a single circle equal to one of them on the other, we obtain a similar result; the inner circle appearing to be oblique to the plane of the other. Again, let the two images be such that the lines of one cross those of the other, the result of their combination is that the outlines alone of each figure are well marked, the colour being a (variable) subjective resultant of the two superposed colours.* If the distinctness or apparent distance of the figures is different, one will appear to be seen through the other. This force of outline, which is illustrated by fig. 17,

FIG. 17.



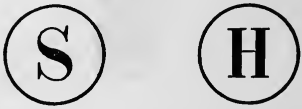
Result.

* According to the researches of Foucault and Regnault, the resultant of different colours seen by the two eyes, although at first variable and inconstant, becomes after looking for some time the same as the resultant to one eye. "Comptes Rendus," vol. xxviii., p. 78; similarly Dove, Poggend. Ann., vol. lxxi., p. 111.

appears to be the effect of contrast. It is a general law of perceptions that they are rendered vivid by contrast or change, and the contrast between two colours is most marked at the bounding line. But it may also happen, in consequence of the power of outline, that the less distinct is wholly suppressed. For an outline to be preserved must bring with it a portion of its bordering colours, as we find in fact occurs.* Thus if we look through two parallel tubes at two shillings laid on white paper, making the visual axes slightly converge so as to separate the images, then if the left eye be adjusted for distant vision and the right for near, we shall see the right-hand shilling complete in its circle of white paper, which will encroach on the other shilling, partially eclipsing it and reducing it to a crescent. By adjusting the left eye gradually to near vision we can make the eclipsed part again visible, but with more or less transparency.

I think it will generally be found that when one image is thus suppressed, the eye is not properly adjusted. This may happen involuntarily when the images are dissimilar, as the S and H in Wheatstone's example. When we attend to the figure in the right eye, the left eye not seeing it, tends to relax its adjustment. But as soon as its image disappears, its sympathy with the right eye begins to restore the adjustment, and then S is seen crossing H mosaicwise. If this new figure fixes the attention the right eye may begin to fall away, and so on. Certainly when the right-hand figure only is visible, if I close that eye I find that the other is not rightly adjusted. If this be the correct explanation of the phenomenon described by Wheatstone, it will

FIG. 18.



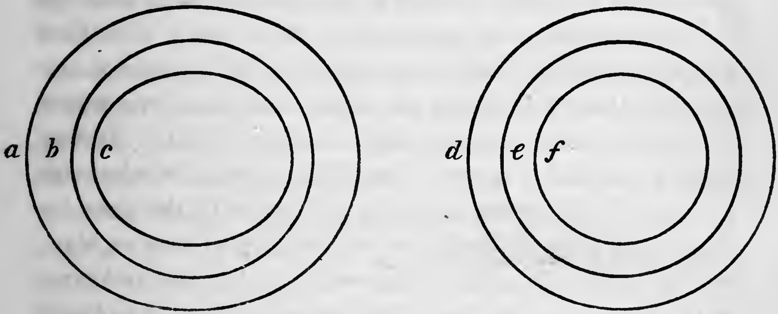
* Cf. H. Meyer, in Græfe's "Archiv," vol. ii., pt. 2, p. 77; and on the whole subject, Panum "Physiol. Untersuch. über das Sehen mit zwei Angen," Kiel, 1858.

be different to different persons, according to the adjustment of the eye when at rest, as well as to the strength of the physical association or sympathy between convergence and adjustment or between the two eyes. And this will account for the circumstance that other observers have in turn contradicted and confirmed Wheatstone's account.

Fig. 19.

Now let us suppose that each image consists of two vertical lines, but at slightly different intervals, or of two concentric circles, the outer ones being equal and the inner slightly unequal. From the pre-

Fig. 20.



ceding facts we might perhaps logically infer that on combining stereoscopically one pair of lines or circles, the remaining pair, being very near, would eclipse one another wholly or partially, or else combine into a thicker line. The actual phenomenon is however very different. It is impossible, one would think, that two intervals visibly unequal should be seen as identical; yet this is what actually happens. As the images approach there is (in some eyes) a tendency to the suppression of one; but as soon as they are united both are seen distinctly, as may be shown by marking the lines. But the plane of the compound figure is not

the same as that of its components. One of the right lines appears behind the other; and this according to a regular law, so that in the case of the circles the inner circle seems to lie not behind or before, but obliquely to the plane of the outer. That in these cases the differing intervals are not united by means of a difference in the adjustment of the two eyes may be shown by employing a figure in which the outer circle is made larger to one eye, and the inner larger to the other, or one such as that above given (fig. 20).*

We see, therefore, that although identical points always give one perception, the converse is not true without limitation. Indeed we should be ill off if it were so, since every point in an object looked at would be confused except those in the line of the horopter. This was however first suggested by observations with the stereoscope. It is easily seen that when the ordinary pictures are combined in the stereoscope it is only those points that are seen at the same level which can at the same moment affect identical points. It was therefore natural to suppose that the appearance of relief was due to a rapid change in the convergence of the axes, by which each part of the picture was imaged in turn on identical points.† This hypothesis was completely overthrown by the experiment of Dove (often repeated since), who found that the relief is seen perfectly when the pictures are illuminated by the electric spark, which lasts only the millionth of a second, just as natural relief is seen perfectly by the light of a flash of lightning.‡ A change in the convergence

* I have used in the figure a circle and an ellipse instead of two circles, as the former are more readily combined, for a reason which will be seen on p. 116, *sub fin.*

† Brücke, in Müller's Archiv., 1841, p. 459. Brewster, Edin. Trans., 1843, p. 349.

‡ Dove [Berlin Monatsberichte, 1841, p. 252.] Volkmann uses a mechanical apparatus ("Tachistoskop") to prove the same thing. This

requires nearly half a second.* It is necessary to remark, that Dove used only simple geometrical figures, so that the relief could not be assisted by the general effect.†

Two experiments of a different kind lead to the same conclusion, and still further illustrate the phenomenon in question. The first (which is August's) is as follows:—When a polished steel rod or a needle is held obliquely to the light each eye sees a bright spot upon it; with both eyes both spots are seen distinctly. If now the needle be made to revolve rapidly in a plane parallel to the line joining the eyes, we shall see not two curves but one, which however will appear inclined obliquely to the plane of rotation.‡ The experiment may be easily made by fixing one needle in the end of a cork as an axis, and another in the side, so as to intersect the axis. Now in this case there can be no question of change of convergence; for the points united have not been affected at the same moment, and it is only by the persistence of the first impression on the retina that it becomes possible to unite it with the other. A similar experiment, and still more striking, is the following. Instead of looking simultaneously at the two stereoscopic projections of a solid or line, if we look first with the right eye at the right-hand line, the left eye being closed, and next closing the right eye look at the left-hand line; then, upon looking

has the advantage that the eyes can be accurately adjusted before the moment at which the pictures were seen. ("Berichte d. Sächs. Gesellsch. d. Wissensch. zu Leipzig," 1859, p. 90).

* 25‴, according to Volkmann, l.c.

† As he himself found it necessary to re-state, (Poggend. Ann., vol. cx., 1860–2, p. 494), in reply to the misconceptions of v. Recklinghausen, which are repeated by Panum in Reichert and Du Bois' Archiv., 1861, p. 68.

‡ August, in Poggend. Ann., vol. cx., (1860), p. 582, where the form of the curve is also deduced.

away to a dark wall, we shall find, if the lines were sufficiently luminous, that the two subjective images unite into one line in relief.* A luminous line may be produced by means of a slit in a piece of card held up before a lamp. Or if we make a screen revolve rapidly between the eyes and a pair of stereoscopic diagrams, so as alternately to cover each, and prevent simultaneous vision, we shall find that the relief is still quite apparent. To eliminate all possibility of the influence of preconceived ideas from the appearance of perspective or any other cause, the following experiment is suggested by Dove.† Looking at the reflection of a flame in a circular-turned metallic plate (*e.g.*, the brass top of a telescope) we see a line of light due to the reflection from the circumference of the circular scratches left by the turning. When seen with both eyes the line appears inclined to the surface. Now, when the plate is illuminated with the electric spark, this line is seen as the path of two sparks, which either start from the outer circumference and meet in the centre or *vice versa.* The cause of this appearance is doubtless, as Dove suggests, that, with the casual convergence of the axes, we do not perceive the illuminations of the centre and of the circumference at the same moment.‡

From these experiments it follows that points may be perceived as one, although they do not affect identical points of the two retinas, but with this peculiarity, that the deviation is accompanied with a perception of depth independently of any change in the convergence of the axes. The limit of admissible deviation is however very small; in a horizontal direction from fifteen to thirty times the least interval sen-

* Prof. W. B. Rogers, in Silliman's Amer. Journal, vol. xxx., p. 387.

† Poggend. Ann., vol. cx., l.c. "Optische Untersuch.," p. 163.

‡ Dove, "Farbenlehre," p. 163. Poggend. Ann., vol. cx., p. 496.

sible on one retina; in a vertical much less.* Professor Panum infers from this the existence of "corresponding circles" (rather ellipses) on the two retinas, such that when any two points in corresponding circles are affected the resulting perception *may be* one, although not necessarily so. As a summary expression of the facts this is at least convenient, although as a theory it may be objectionable.

Adopting this language, it is to be observed that the "corresponding circles" are much smaller near the central spot of the retina than towards the periphery.† This increased tendency to coincidence is of great advantage in binocular vision, as, in consequence of it, not only are the lateral parts of the field less confused, but our perception of depth is materially assisted.

According to the view presented in the last chapter, the conditions of perception of depth with one eye are precisely analogous to those just described; and this is some confirmation of the correctness of this view.

That the binocular perception of depth is something specific and unique will hardly be doubted, now that all are familiar with stereoscopic effects. These furnish us, in fact, with a proof of the most tangible kind, that there is an element in this perception more powerful than suggestion; for the difference has a commercial value. Its extreme delicacy is shown by the practical use to which, after the suggestion of

* Panum, "Sehen mit zwei Augen," p. 55. He found that two pairs of lines could be stereoscopically united (with the naked eye) when the difference of the intervals was 2 to 3 millim., and by some eyes even at 4, but less constantly, the distance from the eye being 460 mm. A difference of 2 mm. would correspond to an interval on the retina of about ·0325 mm., or ·0134‴ (lines), if the deviation is divided between the two lines of the pair, as at the limit it probably is. Panum's own calculation is incorrect. It is hardly necessary to remark, that this union of two lines is totally different from confusion.

† See Volkmann, in Græfe's "Archiv.," vol. v., pt. ii.

Dove, it has been applied—viz., the distinction of a copy, forgery, &c., from the original. When a forged bank note, for instance, is placed in the stereoscope with a genuine one the least difference in the intervals of the words or letters is sufficient to convey the idea of an inequality of surface.* Even impressions of the same die in different metals can be distinguished by the same method. This sensibility to a difference otherwise imperceptible, proves, if further proof were needed, that the relation of identity between the two retinas is natural, not acquired; and further, that the apparent depth is not an idea suggested by the difference which is not itself perceived.

If anything were wanting to prove that the special perception of relief belongs to binocular vision, it would be supplied by instances in which relief is suggested in vision with one eye, and disappears when both are used. If we describe on the same base the two stereoscopic projections of a six-sided pyramid in different colours (blue and red), viewed with one eye, the figure will convey the idea of a double pyramid. But if two such figures are united in the stereoscope we can see nothing but two stars in a plane.

The following experiment illustrates the independence of these phenomena on association. If we lay a thread, or slip of paper, on each of a pair of stereoscopic pictures, by adjusting its distance from the lines of the picture according to the preceding principles, we may make it appear, when viewed through the instrument, either behind or before the paper, notwithstanding the inconsistency of the appearance both with our knowledge and with the effect of the picture.

Indeed, that the perception of relief in these experiments is not the effect of association I have a convincing proof in my own person. The focal difference between my eyes is

* Dove, in Poggend. Ann., vol. cvi., p. 657; vol. cx., p. 498; and "Farbenlehre und Optische Studien."

very considerable: the data are as follow:—The near limit of distinct vision with the right eye is ordinarily 5 inches, or with considerable effort 3½, the furthest 10½; when at rest it is adjusted for about 8. The nearest distance of distinct vision with the left eye is ordinarily 8, and with the utmost effort about 5½, the furthest infinite; when at rest it is about 18 inches. When reading, &c., I generally hold the book at a distance of about 8 inches, so as to suit the right eye; and I have found by repeated experiments, that in that case the left eye is then in the position of rest; so that if the right is closed, or part of the page hidden from it, it requires an effort to bring the left eye to a proper adjustment. If the distance happens to be a little above this, the left eye assumes the appropriate adjustment, but the right is at rest. The convergence is however correct, and vision more distinct than with one eye only.* Now, if I endeavour to combine a pair of the common stereoscopic pictures (without an instrument) by convergence, the image in the left eye becomes indistinct, and at last disappears; if by divergence, that in the right. It is only by making the distance of the pictures very small that I can effectively combine them. With the ordinary lens stereoscope the image in the right eye is of course quite indistinct, and I have no proper binocular perception of relief whatsoever. It appears obvious from these facts, that the association mentioned could not exist in my case, as the two eyes scarcely ever see distinctly together. In order to equalize them it is necessary to use a concave lens with the right eye. Now, the very first time that I used such a lens in looking through a stereoscope, or in looking at distant objects, the perception of relief was extremely

* It may be remarked, that when the page is moved gradually away there is a momentary, but scarcely observable, indistinctness at the point where it passes from the range of easy vision of one eye to that of the other, but no sensation whatever in the eyes themselves.

vivid, and time and association have added nothing to its first vividness. The relief presented by real objects becomes also much more vivid by the use of the lens.*

We have already, then, obtained not only a perception of distance, but a step towards the measure of it. But all this refers to the eye at rest. It is difficult, however, to keep it so when looking at such figures as those described. The momentary perception of depth suggests and excites a motion of the axes, and as far as our present knowledge extends, the convergence of the axes is a principal, if not the principal means of perception of relative distance. This is familiarly illustrated by the difficulty of threading a needle (sideways) or snuffing a candle, without using both eyes. Its importance may be shown by a very simple experiment. If we place a number of threads vertically at slightly different distances from the eye, arranged for example in zigzag, no difficulty will be found in perceiving the relative depth; but if they are placed horizontally they will all appear to be in the same plane. There is, in fact, nothing in this case to determine

* This inequality, I may observe, is no anomaly. It is remarked indeed by a German writer (v. Hasner) that the equality in the power of the eyes is matter of daily experience. It would be nearer the truth to say that the contrary is matter of daily experience. The experience of persons who have never made the comparison must go for nothing. The unity of action of the two eyes is so complete that the greatest inequality of power, even to total blindnesss of one eye, often exists unnoticed till revealed by accident. The testimony of scientific observers is, that inequality, if not the rule, is at least very frequent. See Lawrence, "Diseases of the Eye," p. 43; Franz, "On the Eye," p. 169, 170, where the evidence of Ross, the well-known optician, is cited to the same effect, [Wardrop, "Morbid Anatomy of the Eye," vol. ii., p. 244]. The following writers occur to me who have noticed such a phenomenon in their own sight:—Airy, Bidder, Fries, Meissner, Panum, and in a less degree, Aubert, Förster, Reid. I have also met with several notable instances amongst my own acquaintances. Great inequality is a frequent cause of squinting. See Donders, "Verslagen en Mededeel. der k. Akad van Wetensch." (Amsterd.), pt. xv., p. 136; "Accommod. and Refract.," p. 295.

the convergence (since any two points in a right line give identical impressions). If however knots or other marks are made on the threads, by which the parts can be distinguished, the distances are again perceived. Again, if we look through a lens $2\frac{1}{2}$ inches in diameter at a figure consisting of lines of different colours, those which are least refrangible will stand out, very decidedly above the others.*

Now it is easy, with the help of a suitable apparatus, to change or even invert the usual relation between convergence and adjustment. The axes may converge to a near point while the eyes are adjusted for parallel rays, or the axes may be parallel while the eyes are adjusted to the nearest limit of distinct vision; and brightness or distinctness may be varied still more easily. Now it has been shown by repeated experiments of this kind, that within the limits of sensible variation of adjustment and convergence brightness and distinctness are quite without effect, and wherever these indications are at variance we are invariably guided chiefly, if not entirely, by the convergence alone, and that without the least hesitation, or any consciousness of discrepancy or uncertainty. An ordinary lenticular stereoscope furnishes the simplest instance of this when the pictures are geometrical figures. And a comparison of the effect obtained by this means with that of a painting, will show in some degree the proportionate power of the different indications of depth. But the following experiment of Sir D. Brewster† will show the effect of convergence most clearly and beautifully. Standing before a wall (say at a distance of three feet), the paper on which has a small pattern, let the axes of the eyes be directed to a point nearer to the observer. This may be done either by looking at a nearer object or by looking at

* Brewster, "The Stereoscope," p. 126; North Brit. Review, vol. xvii., p. 200.

† Transact. Royal Soc. of Edinburgh, vol. xv., p. 663; Philosoph. Mag., vol. xxx., p. 305.

different parts of the pattern with the two eyes. As soon as the paper is clearly seen in this way, it will appear to stand suspended in the air, nearer to the observer than the wall, and somewhat convex. If he move backwards, or from side to side, it follows his motion. If the axes are made to converge to a point beyond the wall, then the paper seems at a greater distance; but this is less easily accomplished, as most persons find some difficulty in looking through a wall as it were.* Sir D. Brewster suggests, therefore, as a good object for this experiment, the seat of a cane-bottomed chair, which can be looked through, and the eyes directed to a point more remote. In this case, as he observes, we may feel an object where we see nothing, and see one where we can feel nothing. We can put our hand through the visible seat, and we see nothing in the place where we feel the real seat. Now, in this experiment we have, on the one side, not only the actual felt distance, the brightness and distinctness, &c., and (when we are familiar with the size of the object) the apparent magnitude, but we have also that focal adjustment which has been connected by an absolutely uninterrupted association with clear vision at this distance; yet the perception is determined in contradiction to all these indications by the convergence alone, and that instantly and irresistibly. Whatever difficulty may be at first felt in this experiment is due to the novelty of the effort to break the long-established physical association between a certain degree of convergence and a certain adjustment; but a very little practice enables us to overcome this. Indeed, in the ordinary use of the stereoscope the natural association is interrupted, though very slightly. Now, so far as there has been association with the idea of distance, it has been more con-

* For this form of the experiment the similar parts of the pattern, &c., to be united must not be more than about 2½ inches apart, or the distance between the centres of the eyes.

stant with the adjustment than with the convergence; for we do not always see with both eyes either near or distant objects; yet this, aided by the actual perception by touch, has not the least effect upon our estimate of distance as long as the convergence is maintained. It simply tends to bring the convergence itself into harmony with the adjustment. A similar process takes place when we unite a pair of stereoscopic pictures by the unaided eye.

In these cases it is to be observed that the apparent (*i.e.*, sensibly apparent) magnitude of the object is proportional to the apparent distance. It is not however inferred from it, but is as directly perceived. This is evident, first, from the experiments already mentioned (p. 57), and secondly, from the fact that our judgment of magnitude at small distances is more correct than that of distance. But the experiments of Professor Wheatstone show in a very striking manner the dependence of apparent magnitude, as well as apparent distance, on the convergence of the axes, and at the same time prove that neither is a mere suggestion of the other. He arranged the pictures in his mirror stereoscope in such a manner that, on the one hand, the angle of the optic axes might be altered, while the image on the retina remained the same, or, on the other hand, the image could be decreased or diminished without altering the angle. This being so, we find that when the convergence alone changes, the object does not appear to move, yet, when attentively regarded at any moment, its distance appears greater or less according to the diminution or increase of the angle. But when the magnitude of the retinal image only is altered the object does appear to move, yet when attentively regarded at any moment it appears to be at the same distance. Singularly enough, in both cases, whether the object appears to move or not, it appears to change its magnitude.*

* Philos. Trans., 1852. These observations were repeated (? independently) by H. Meyer, who gives some important measurements and

A singular appearance, which exemplifies the same principles, may be observed when we look steadily at a locomotive engine rapidly approaching us. Although moving continuously its apparent distance changes *per saltum*, while its apparent magnitude seems to increase and diminish alternately. The eye, not being able to follow the rapid motion of the object, alters its adjustment *per saltum;* but the retinal image increases continuously. Hence, as in the preceding experiments, the engine seems to move and grow in size continuously, assuming however, by fits, a nearer position and a less magnitude.*

From these and the like observations we may conclude that apparent or perceived magnitude and apparent distance are together determined by the same visual sensations as determine the convergence and adjustment of the eyes.

We must notice certain facts, which may appear to be inconsistent with what has been remarked of the effect of

calculations (Poggendorff's Annalen, vol. lxxxv., p. 198). The pictures are set movably on arms, which turn on a pivot between the two mirrors. By moving the pictures on the arms the reflected images move nearly in the line of vision; and when the arms revolve slightly the reflected images approach or recede from one another. To fulfil these conditions accurately each arm ought to move on a separate pivot in the focus conjugate to the centre of the corresponding eye. But if the common pivot is at a distance of $1\frac{1}{4}$ inch from the intersection of the reflecting surfaces, and in the medial line, the result is sufficiently accurate. A simple instrument for altering the convergence while looking at a single object is suggested by Rollett, consisting of two thick plates of glass joined at a variable angle, and which therefore displace the object by refraction in opposite directions. ("Physiol. Versuche," in the Vienna Sitzungsberichte, vol. xlii. (1860), p. 488; Moleschott's Untersuch., vol. viii., pt. ii., p. 178.) Halske has adapted movable figures to the ordinary stereoscope. We may dispense with an instrument, and unite with the unaided eyes two similar objects, *e.g.*, two pencils, which we can bring nearer to each other or to our eyes, or the contrary, as we please. A pair of tubes would facilitate the operation.

* The phenomenon of an object appearing to approach, and yet not changing its distance, is exemplified in the shadow pantomime, when the shadows increase in magnitude.

convergence in determining apparent distance in opposition to adjustment. For example, if by an adequate alteration of convergence we run into one the images of two similar real objects, such as two pencils or two columns, the resultant image appears indeed a little nearer or farther than its components, but not at all in proportion to the change of convergence. The account of the matter seems to be this. We have seen that the perception of distance by a single eye requires the presence of a varied field. Now, in the case of the wall-paper, &c., the field is so nearly plane and uniform as not to supply the requisite variety of impressions to a single eye, while, for the same reason, it is not distorted by the change of convergence. Accordingly, if this condition is not fulfilled the illusion does not occur. This may take place if there is a window or door near the part of the wall looked at, or if there is a table before it. In the experiment last mentioned the change of convergence affects only a single object, while the general perspective of the field is still determined by the visual sensations belonging to each eye separately. These observations therefore confirm the view above taken, that apparent distance is determined, not by muscular but by visual sensations. These visual sensations are, as we have seen, of an analogous character in vision with one and with both eyes. In both cases also the perception is dependent on the presence of a certain variety or discrepancy in the impressions. In the case of binocular vision this discrepancy is intensified, and the delicacy of the perception is proportionately increased. The changes in distinctness, &c., occasioned by the motion of the eyes, are also analogous and more considerable; but from the experiments mentioned above (with the electric spark, &c.), it is clear that motion, although an auxiliary, is by no means a necessary condition of the perception of depth.

Some of the special visual sensations attending the bino-

cular vision of real objects may be here noticed. Suppose, for instance, two balls, A, B, to be fixed, one behind the other. When we fix our eyes on the nearer, A, the whole of it is seen distinctly with both eyes, while an indistinct image of the other ball, B, is formed on each retina, but on points not corresponding, the image belonging to the right eye appearing on the right of A, and that to the left eye on the left, each in a place which to the other eye is occupied by a different object; it is therefore doubly indistinct. On the other hand, when we view the remoter ball, B, then part, or the whole of it, may be distinctly seen, but not generally all with both eyes. Part of the distinct image in each eye is formed on points corresponding to the indistinct image of the nearer ball, A, in the other eye. This both renders the latter image still more obscure, and gives the impression of looking through it at the other. In this case, the image of A, which belongs to the right eye, is seen on the left of B, and *vice versa*. Binocular vision is thus not only the most perfect means of evoking the conception of distance, but it involves a more definite suggestion of the relations of nearer and farther, and of the effort required in "looking at" an object, especially when the appearances just mentioned are combined with the distinct perception of depth, which we have already seen is obtained with unaltered convergence.

It is perhaps worth observing that the field of double vision diminishes with near convergence. The field of view of each eye is about 150°—namely, 60° inwards and 90° outwards. Now, when the axes are parallel the combined field extends to 180°, of which two-thirds, or 120°, are common to both, but when the axes are inclined at an angle of 60° the total field is reduced to 120°, and the common part is 60°. It has been before mentioned that the field of distinct vision with each eye is greater in near than in distant vision;

and from what is now said it appears that in binocular vision the relative extent of the distinct portion of the field increases still more rapidly than its absolute extent. For the rest, it is too obvious to require special notice, that in the vision of real objects the existence of depth or change of distance occasions precisely those visual impressions which, in the experiments above mentioned, convey the perception of depth or distance.

A different question is, what is the sensible limit of convergence? This may be sought in two senses, either what is the least angle which can be visually distinguished from parallelism of the axes, or what is the greatest angle compatible with distinct single vision of distant objects? Now, when any point is looked at with the axes parallel, the angular separation of the two images is equal to the angle subtended at that point by the interval between the eyes, viz., about $2\frac{1}{2}$ inches (p. 53). Hence, from the sensible identity of the two retinas, the former limit ought to be the same as that of the separation of two lines by a single eye. For an ordinary eye this may be placed at about $35''$, corresponding, for an interval of $2\frac{1}{2}$ inches, to about 400 yards. But the latter limit depends on the magnitude of the "corresponding circles" spoken of in p. 117. Assuming Panum's data, this angle will be about $7'\ 28''$, corresponding to a distance of about 32 yards. The distances found by direct observation agree pretty well with these, being, however, less in proportion to the diminished clearness of distant objects. The former limit is best found by observing an object within the angle of the visual axes, the images of which will therefore be on opposite sides of the point looked at; but in finding the latter limit this must be avoided, since two images at opposite sides of the middle points of the respective retinas can never convey a single impression. Within this limit the depth or difference of distance at which we

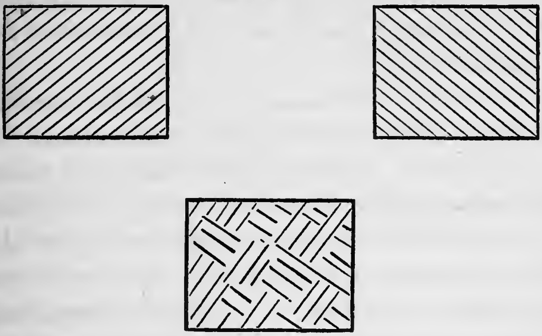
can see objects distinctly with the same convergence, is for near objects proportional to the square of their distance from the eye, and for those near 32 yards, inversely as their distance from this limit.*

Since images not falling within corresponding circles must convey distinct impressions, the question occurs—How does it happen that we are so rarely conscious of seeing double? "You may find a man," says Reid, "that can say, with a good conscience, that he never saw double all his life; although, when the experiment is made, and he attends to the appearance, he will at the first trial see double." It is certainly not because when our attention is fixed on a distinct image "we disregard the images of objects not in the visual focus" (Carpenter); for if we hold up two fingers before one eye, and look beyond them, we shall see both, though indistinctly. But first, these indistinct images fall on the lateral parts of the retina, which are much less sensitive to differences, both of place and colour, the further they are from the centre. When the two images are on opposite sides of the centre, they are readily distinguished; whence the old opinion, that double vision was limited to objects situated within the angle of the optic axes, or its vertical angle. But, even in this case, unless the objects are very narrow, the two images will overlap, and the only effect will be to render the object more indistinct. Secondly, each image falls on points corresponding with those of a dissimilar image on the other retina. If both objects were seen, they would appear in the same place, and consequently doubly indistinct. It so happens, however, that when identical points receive dissimilar impressions, we do not in general perceive both simultaneously.

* A point at the distance d being looked at, the depth beyond it corresponding to a given angular separation of the two images is $\frac{d^2}{a-d}$; a being the distance at which this angle is subtended by the interval between the eyes. The depth on this side of d is $\frac{d^2}{a+d}$.

It is easy to convince oneself of this by trying to combine stereoscopically two sets of parallel lines, one vertical and the other horizontal (fig. 21), or two very different letters, such as S and H.* As there is usually some inequality in the power

FIG. 21.



Result.

of the two eyes, if both images are equally indistinct, that in the stronger eye predominates to the total exclusion of the other. When however the object doubled is nearer than that looked at, the indistinct image on each retina corresponds to a part of the distinct image on the other. In this case the distinct image usually overpowers the other, as may be readily shown by experiment. When the two indistinct images overlap, so that a part of the remote field is hidden from both eyes, we see the intervening object unmistakably, but of the magnitude and figure of the overlapping part. This may be verified by holding the hand before our eyes and looking beyond it. The more distant the object looked at, the narrower the hand will seem.

It must be remarked, further, that it is difficult, without practice, to look at one object and attend to another; the question therefore is, why the double image is not distinct enough to attract our attention when we are not looking at

* See the subject fully discussed by Panum, "Physiologische Untersuchungen über das Sehen mit zwei Augen;" Kiel, 1858; and compare Wheatstone, Philos. Trans., 1842.

it? and this is fully answered by the preceding considerations. If from any cause our control over the motion of the eyes is impaired, so that we fail to direct the axes to the object looked at, then the double vision does attract our attention. This is the true reason why the drunken man sees double, and why the same symptom so often precedes cerebral attacks.*

The same reasons which have been given for regarding the adjustment as a reflex action, apply with equal or greater force to the motion of convergence. Indeed there appears to be an organic connexion between these two motions, so that a certain effort of near adjustment is accompanied with a corresponding motion of convergence. Hence even when one eye only is used, any change in its adjustment is accompanied with a change in convergence. This is easily proved directly, by voluntarily altering the adjustment of one eye while looking at any object, the other eye being covered, and then opening the covered eye, when the images will be found separated in proportion to the inaccuracy of the adjustment. In normal eyes these associated adaptations always harmonize; but in hypermetropic eyes, in which the retina is too near the lens, a proportionately greater effort of adjustment is required for distinct vision, and the convergence accompanying it is therefore excessive. To avoid the double vision which is thus produced, one eye turns still farther inwards, so that a convergent squint is produced.† It is possible however, by practice, to dissociate these motions to a considerable degree,‡ and then the changes

* Double vision is best observed with luminous points, *e.g.*, pinholes in a card held up towards a lamp.

† This cause of squinting was first discovered by Donders, "Refractie-anomalien oorzaken van Strabismus." Versl. en Meded. der k. Akad. Amsterd., 15e deel, p. 134. "Anomalies of Accommod. and Refract.," p. 294.

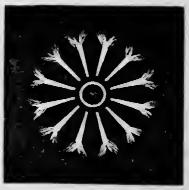
‡ The limits of this power (for different degrees of convergence) have been investigated by Donders. Græfe's Archiv, vol. vi., pt. i., p. 84., and "Anomalies," &c., p. 110.

in the convergence are found to be partly involuntary. Thus, in combining stereoscopic pictures with the unaided eye, as soon as the images have been brought nearly to coincide, they run together, as if by attraction, and remain united without further effort, even though the figures are slowly moved to or from one another. It appears probable then that the similarity of the images is at least in part the occasion of a change of convergence. Indeed, something similar to this occurs in ordinary vision; for the concomitance of adjustment and convergence, just spoken of, is not perfect. If we have been looking at an object with one eye only, we shall generally find, on opening or uncovering the other, that it is not directed exactly to the same point, nor is it always adjusted accurately to the same distance. Hence we may infer that, in ordinary vision, the adjustment of the eyes is attended with a degree of convergence nearly, but not exactly, adapted to the distance of the object, the deviation being corrected instantly and unconsciously as in the stereoscopic observations. Hence it is possible for the power of adjustment to be deficient in a considerable degree, as in the case of both myopia, or shortsightedness, and presbyopia, without impairing the singleness of vision. But in ordinary vision we are never aware of the double image, and even when we have learned to observe it, we do not perceive that the impressions belong to different eyes. In fact, by the sensations of sight alone, we could never discover that we possessed two eyes.

The involuntary character of the process is remarkably illustrated by a case recorded by Græfe, in which the central part of the right retina was insensible. In the first experiment, while the patient looked at the flame of a candle, to which both eyes were directed, a prism was held before the left eye, so as to throw the image of the flame outwards. This eye then turning inwards in order to receive the image

centrally, the right eye, by association, turned outwards, but at once returned to its former position, so that a convergent squint arose. Probably, says Græfe, the eye turned until the image of the flame fell on a sensitive part, and then the excitement of an eccentric part of the retina sent back the eye in order to maintain single vision. But the patient, although practised in scientific observation, could not be made to perceive the double image, even when differently coloured glasses were used to assist the distinction. To test this explanation of the phenomenon, Græfe next placed the prism before the right eye, so as to throw the image outwards. This eye now constantly turned inwards so as to receive the image on the insensible central spot, while the left eye retained its position. In this experiment also the double image was not perceived; but that the image fell on a sensitive part of the retina, was shown by covering the left eye and placing the prism in the same position as before; whereupon the flame was at once perceived.* In this case the convergence was affected apart from the adjustment.

FIG. 22.



Another illustration in point is supplied by my own sight. When I view a luminous point, at a distance of two or three feet or more, with both eyes, the two images are quite dissimilar; that belonging to the left eye being a point, that to the right a star-like figure, as represented in the margin.† Now these two images appear separate, that belonging to

* Græfe's Archiv für Ophthalmol., vol. v., pt. 1, p. 128.

† The immediate cause of such starlike figures of diffusion has not been determined, but it is certainly in the lens. See Donders, Græfe's Archiv, vol. vii., pt. 1, p. 186. Helmholtz, "Physiol. Optik," p. 138. Young, Philos. Trans., 1801, p. 43. The last two authors give figures from their own observation. Donders has also published a figure, which I have not seen, in the "Nederlandsche Lancet," vol. ii. The figure described in the text, with the exception of the central circle, corresponds

the right eye being about 15′ to the left of the other, and in this position they have not the least tendency to approach, but readily separate further. On the contrary, when an object not luminous, or which, if luminous, has a sensible diameter, is viewed at a moderate distance by both eyes, it is seen distinctly as one. This is not by the disappearance of one image, for I can readily separate the two images in either direction, but they naturally and easily fall together again. It may be inferred therefore that in the former instance the appropriate convergence is assumed simply as a concomitant of the adjustment of the left eye, there being nothing in the visual impression which can determine it. In Græfe's case, on the other hand, the determining power of the visual impression easily overcame the muscular association.

We conclude, therefore, as in the former instance, that whatever the visual sensation may be, since it determines the adjustment and convergence with such accuracy and celerity that we are not aware, even for a moment, of the existence of a double image, it is also sufficient to determine with equal accuracy a perception of the distance corresponding to this adjustment and convergence. Now we have before proved that such a perception does in fact exist, the laws and conditions of which are wholly independent on those of touch; and it has now been shown further that these laws and conditions are identical with those of the motions just mentioned, although independent on the actual motions themselves. It may be said therefore to be demonstrated, that the perception of distance is in the strictest sense a proper and original perception of sight.

very closely with Helmholtz's figure of the direction of the fibres in the lens, in which however the number of rays is only six. But as the rays in the opposite surfaces of the lens are alternate, this does not interfere with the analogy. The arrangement of these fibres, it must be observed, is not the same in all eyes.

CHAPTER IX.

WHAT WE LEARN FROM EXPERIENCE OR ASSOCIATION.

It remains to say a few words on the proper function of association or experience in assisting our visual estimate of distance. In the first place, since distance may be measured either by the eye or by touch, two senses which have no common standard, it is obviously necessary to establish, by experience, some relation between them. Although the eye possessed the most perfect power of perceiving distance, it could not possibly convey any idea of the amount of walking necessary to pass over it, no more than touch, if it had the like power, could convey the idea of the change in the visual appearance corresponding to an increased distance. One would have thought this evident. Yet, because Mr. S. Bailey admits that Cheselden's patient would have to compare his visual sensations with actual experience before he could know that the distances which he saw corresponded to those previously known by touch, Mr. Mill declares that this seems like a surrender of the whole question. "If we saw distance we should not need to learn by experience what distances we saw." He might just as well say that to admit that a person can be taught to distinguish musical tones with accuracy, is to admit that they are not perceived by the ear. "If we heard the tones we should not need to learn what tones we heard." Certainly we are no less beholden to experience for the accurate knowledge of the distances we move through.

Secondly, as the eye gives us distance in two ways—laterally and in depth—experience is no doubt necessary to bring these to a common standard. It is hard to conceive how this could be dispensed with on any hypothesis. Certainly Mr. Mill and Mr. Bain cannot object to it, since on their theory we have the following distinct and variable standards of length. First, the statical given by simple touch; this varies greatly in different parts of the body, in the proportion of one to sixty; and even in different parts of the hand, the variation is in the ratio of one to five.* Secondly, the dynamical standard of the motion of the hand, which is also, as we have seen, indefinite in the highest degree. Thirdly, another dynamical standard, the exertion of walking, which is, indeed, the least indefinite of the three, but still liable to great variation.

Thirdly, we must trace to association the power of estimating the distance between points which are seen distinctly without variation in the adjustment or convergence; that is to say; first, all increase of distance beyond a certain limit (the limit of convergence); and secondly, all intervals within this limit, which bear too small a proportion to their distance from the eye. The signs by which our judgment is affected in such cases have been already alluded to. Mr. Mill appears not to have imagined any besides brightness and dimness; and Mr. Mansel also attributes a large part to these, while adding to them distinctness of outline. In the second class of cases just mentioned, neither brightness nor distinctness can, generally speaking, come into consideration at all. The variations of light and shade simulate the degrees of diffusion of the retinal image. It does not, however, belong to my purpose to enter into detail with respect to these signs.

* E. H. Weber, "Tastsinn," ["Annotationes de pulsu, &c.,"] Graves, "Studies in Physiology," p. 186.

Beyond the point at which the variation in the impression ceases, and with it the direct perception of distance, the view is carried on by means of continued lines and objects of known magnitude, &c., by the help of which we estimate greater distances. Where there are few or no intervening objects, as when we look over a uniform plane, or the surface of the sea, distance is underrated. In this there is no question of a suggested idea mistaken for a perception; the idea suggested by the visual sensations is itself a visual one, in fact only a modification of a perception actually present. It is analogous to the suggested variations in the perception of weight mentioned above (p. 39). Nevertheless there is not the least difficulty in distinguishing the part of the distance which is estimated from that which is directly perceived.* They differ as the depth suggested by a painting and by a stereoscopic combination. We observe also, that within the limit of perceived distance, the apparent magnitude varies very slightly; while beyond this limit it diminishes regularly as the distance increases. Where there are no intervening objects, the whole of the remote field appears at a uniform distance, corresponding pretty closely with the "limit of adjustment." Hence we obtain an explanation of the apparent magnitude of the visible heavens.† An angular interval of 1°, if seen as at a distance of thirty-eight yards, would appear as a length of two feet; seen as at thirty yards, would appear about a foot and a-half. Now the belt of Orion, which is $2\frac{3}{4}$° nearly, appears about three or four feet in length when near the horizon; the small interval between Alcor and Mizar, in the Great Bear, which is about 12′, appears about four inches. I mention in preference those intervals with respect to which there is no disturbing cause such as brightness. Even in the case of the moon, however, the same measure holds nearly; her angular diameter being 32′, and

* Cf. p. 41. † See pp. 55, 56.

her apparent linear diameter about one foot.* Near the horizon all intervals appear larger in consequence of the intervening objects on the earth's surface conveying the measure of a greater distance. This is doubtless the true explanation of the greater apparent magnitude of the moon when near the horizon, which, rejected by Berkeley, has been adopted by R. Smith, Reid, Brewster, &c.† Smith observes, that when we endeavour to bisect with the eye the arch from the zenith to the horizon, the point fixed upon has an altitude of about 23° only; and if we compare the interval between two stars near the horizon, with that of another pair near the zenith, it will be found that when they appear equal, the latter will be really three times as great as the former. Since, then, all intervals appear less towards the zenith, the diminished brightness of the horizontal moon is not the cause of her apparent largeness. We find, in fact, that if the moon is looked at through a tube, she does not appear larger at the horizon than elsewhere. With this phenomenon may be compared also that mentioned by Humboldt, who remarks (speaking of stars being seen in the daytime through the shaft of a chimney):—"The chimney-sweepers, from whom I have inquired, say pretty uniformly . . . that at night the sky, seen through tall chimneys, looks quite near, and the stars seem larger."‡

It may perhaps occur to the reader that this account of the increase in the apparent size of the moon, &c., directly

* The apparent largeness of objects seen in a fog may be accounted for on the same principle, the general indefiniteness of outline not supplying the requisite variety in the impression.

† Smith, "Optics," Art. 163, Rem. 302, sqq. Reid, "Works," p. 191. The ratio of the moon's apparent diameter at the horizon to that at the zenith, according to Smith, is as 100 : 30. On the apparent form of the visible heavens see Drobisch. Berichte der k. Sächs. Gesellsch. zu Leipzig, 1854, p. 107.

‡ "Cosmos," vol. iii., p. xxii., note 110. Sabine's ed.

contradicts what was remarked a moment before about the constancy of apparent magnitude within certain limits. The contradiction is only apparent. It is beyond question that the perceived distance and magnitude are in some degree dependent on the number of intervening objects. We have seen indeed that the variety of the impressions produced by these is an important or rather an essential element in the perception proper of (the measure of) distance. Now it is a general law that the difference between two sensations or perceptions appears greater when it is subdivided; but in the case in question there are two special reasons for the effect ascribed to intervening objects. One, that it is only by means of these that the peculiar stereoscopic effect can be produced either with one or both eyes, and the other, that the variation of sensation for the last sixteen yards of thirty-two is not greater than that for the preceding five, or for a couple of inches a little nearer. The delicacy of the perception diminishes very rapidly with the distance, and beyond ten or fifteen yards it is liable to considerable error. Hence the small variation in apparent magnitude even within the limits mentioned. But this leads to the next consideration.

When we speak of a visual measure of distance, as, for example, of an object appearing as at a distance of ten yards from the eye, it is not meant that the eye tells us directly that this distance is ten times that of an object at one yard. To insist on this would be to suppose that if an interval makes any impression on the eye at all, it must produce the same sensation at whatever distance it may be. Now this is certainly not necessary, and we know in fact that it cannot be the case. Suppose two objects, one ten and the other twenty feet distant, the interval produces a certain impression in the eye; but this is by no means the same as would be produced by the interval between five and fifteen

feet; still less is it like the impression corresponding to the first ten feet. The same impression then corresponds to a different real interval, according to the distance of the starting point or base from the eye; as in the case of weight, the same sensation corresponds to an ounce or a pound, according as the weight previously felt was measured by ounces or pounds (p. 40), or rather perhaps as the same sensation corresponds to a different weight, according to the position of the arm. It would appear therefore that experience is necessary to enable us to compare intervals at different distances, or refer them to a common standard. The apparent distance of the initial point itself admits, theoretically, of great indefiniteness, since we have not found any means of perceiving the amount of distance from the eye absolutely, except perhaps in binocular vision. In natural vision however there is a proximate visual base, referred to about the level of our own body. All these things considered, it is certainly remarkable that the perception of distance, absolute and relative, is so definite and uniform as experience shows it to be.

It is scarcely necessary to repeat that experience and memory are primary conditions of every estimate or measure of distance, since such measure is nothing but a comparison with previous perceptions of the same kind (p. 8).

CHAPTER X.

CASES OF PERSONS BORN BLIND.

In order to complete our argument we must now proceed to test the Berkeleian theory by an examination of the cases in which the phenomena of sight are given most pure and independent—viz., those of infants, of persons born blind, who have been enabled to see, and of the lower animals. I shall commence with the case of the blind. This is appealed to by some metaphysicians with confidence, by the more cautious with doubt, owing to the inexactness with which the necessarily difficult observations have been made. Diderot remarks, that "to prepare and question one born blind would not have been unworthy of the combined talents of Newton, Descartes, Leibnitz, and Locke." We cannot wonder, then, if the existing histories are few and unsatisfactory. But there are other reasons why we should not require too much from the visual perceptions of such persons. We have seen that two most important means of the perception of distance are the adjustment of the lens and the varying convergence of the axes. Now, nearly all the recovered blind had lost the lens, and therewith all power of adjustment; and all, without exception, had of course at first the sight of one eye only; and although in some instances the second eye was subsequently operated on with success, yet no special observations seem to have been made in such cases. It is further to be remembered, that "the first visual impressions which one born blind receives after couching do not constitute vision" (Platner).

I have another remark to make before proceeding to de-

tails. The suggestion of Locke and Molyneux has induced English surgeons in these cases to select the experiment with the globe and cube as a test of the perception of solidity. The patient having first handled two such bodies, is then allowed to look at them, and desired to say which is the globe and which the cube. And it has been held with wonderful unanimity that if the recovered blind could not recognise these at first sight, this would be a clear proof that sight gives us little or no idea of the "real figure and magnitude of objects."* Now the fact is, it would prove nothing of the kind. Let it be remembered, first, that even a perfect eye could not at a single glance recognise so much as the outlines of the respective figures. To do so would require a careful ocular examination, and an accurate study of the corresponding ideas of touch. Let the reader reflect how much handling and consideration of an unfamiliar form would be necessary, if the eyes were shut, before we could obtain anything like an accurate idea of its visible appearance. If the figure is a complicated one we could not do it at all; and this after so many years' experience. What would it be if the sense of touch were new to us? And yet, if a man or a child who has never seen before does not at once form an accurate conception of the tactual appearance of the objects seen, this, we are told, is a proof that sight does not convey an idea of the real figure at all. In the instance in question the problem, as usually stated, is not to identify the cube seen with the cube felt, absolutely; but the man is supposed to know that one of the two bodies before him is a cube and the other a sphere, and he is only required to find a visual difference of the same kind as the known tactual difference

* Sir W. Hamilton is an exception. See his "Lectures," vol. ii., p. 177. Note on Reid, p. 137. According to Berkeley, it would prove that visible figure is specifically different from tangible figure.

between them. Now, suppose he has acquired by touch the notion of a straight line, this notion is suggested by all the edges of a cube, and is wholly excluded by the sphere. But it cannot be questioned that the idea of a straight line can be obtained by sight, and that in whatever way the cube is looked at it is bounded by straight lines, which the sphere does not present in any aspect. This furnishes a characteristic quite sufficient for the identification. But sight gives also, I will not say the idea of an angle, but the contrast of continuity and interruption. When the eye traverses an edge of the cube it comes to an abrupt stop, and the look must turn in a different direction several times in the circuit, and a similar change occurs when the cube is handled. But the globe gives to both senses the idea of a continuous uniform outline. Hence we conclude, without reference to the perception of solidity at all, that if the blind are capable of acquiring the ideas in question, they will, on being made to see, be competent to name correctly the globe and cube which they have previously felt. Consequently, if upon being fairly examined they appear incapable of doing so, it will follow that the defect is not in sight but in touch; not in their new sense, but in their old ideas.

A striking illustration of this occurs in the account of a boy couched by Sir Everard Home. His vision after the operation was not more perfect than that of many persons blind from cataract is before any operation. When an object was bright-coloured and illuminated by a strong light, he could make out that it was flat and broad, but not in less favourable circumstances. Now when he was shown a square he could not find a corner until one was pointed out to him, and then he saw it, and could find another at the end of the same side. He was therefore capable of distinguishing an angle by sight. Yet, when the opposite side of the same card was shown to him,

being of a different colour, he could not tell whether it had corners or not. Not that the power of discerning angles on one side of the card was less than on the other, but no clear ideas had been acquired from observation of the first side: the notion of an angle had not been abstracted from the colour that accompanied it. Now if there was a difficulty in recognising the same visible figure under a change of colour, how would the difficulty be increased if the figure were presented in combination with the sensations of a different sense, wrapped up in them, so to speak, and severed from those which had hitherto accompanied it? Whatever ideas of figure the blind can possess have been invariably associated with sensations of touch; and there has been little or nothing to suggest their separation. In the case of sight, the variation of colour compels us to abstract the idea of figure from that of colour, but in the case of touch there is no similar variation. In fact, in the tactual idea of an object, the most obtrusive qualities are those of hardness or softness, roughness or smoothness, adhesiveness, dryness, weight, &c.; the notion of figure is at best very obscure and indefinite. But with sight it is quite otherwise. The component ideas of a visible object are simply figure (magnitude) and colour. A visible object must be seen and thought as having some figure; not so a tangible object. Figure, as already shown, is an essential idea of sight, and at best belongs to touch only accidentally. We must not therefore suppose that the idea of a globe or a cube has been already generalized and abstracted from the sensations of touch, so as to be recognised when presented under new conditions. Nor, on the other hand, are we to expect that on first seeing, the figure of an object can be at once contemplated apart from its colour. Yet these are the suppositions tacitly made by all writers on the subject. Now, to identify an object of any kind as seen with the same object hitherto known only by

touch is to identify a body possessing a definite figure, magnitude, and colour, but of which we know not whether it is hard or soft, rough or smooth, heavy or light, &c., with a body known by these very qualities chiefly, possessing no colour, and a very indefinite figure, if any. Is this so easy a problem as to be solved at the first blush by a youth or child who has never learned to analyse his ideas, and that, too, when his new sense is so extremely imperfect?

We may conclude therefore that the capacity to recognise two such bodies as the cube and globe in the case suggested would be proportionate to the clearness of the previous ideas; and we shall find this confirmed by the cases mentioned hereafter. With respect to the power of discerning solidity, it is obvious that it can be fairly tested only by considerable distances. Even if the lens were present, and capable of adjustment, the experiment ought to be made with a depth exceeding what has been called above the "accommodation line," and time should be allowed for the examination. Where the lens is absent every line in the retinal image must be diffused, and it is at least possible that the conditions of the perception may be wanting. On no hypothesis could it be expected that a person newly seeing could affirm more than that the two bodies mentioned were solid figures, circular and quadrangular respectively.

With these observations, I shall proceed to give some account of the principal cases on record,* beginning with

* They are as follow:—Cheselden, Phil. Trans., 1728; Bortolazzi, "Dissertazione sopra una cieca nata guarita," Verona, 1781; Grant, Lichtenberg's Magazin, vol. iv., p. 21, (Gotha, 1786); Ware, Phil. Trans., 1801 (two), and "Treatise on the Eye;" Home, ibid., 1807, (two); Wardrop, ibid., 1826; Franz, ibid., 1841; and Medical Gazette, 1840–1, vol. ii., p. 956; Stafford, Med. Chirurg. Trans., 2nd series, vol. vii., 1842; F. C. (*i.e.*, Florent Cunier), Annales d'Oculistique, vol. x., 1843, p. 144; Duval, "Reflexions sur les premieres impressions d'un aveugle né rendu clairvoyant," ibid., vol. xiii., 1845, p. 97; Trinchinetti, "Gior-

Cheselden's, as the oldest and most familiar. His account, however, by no means deserves the character often given to it of "highly accurate and philosophical," and is on many points inconclusive. At the time of the experiments, the patient (a boy of thirteen) had the sight of only one eye, and that had lost the lens. How could any negative evidence from such a case be of the slightest value in the present argument? It is not imagined that we perceive distance by magic without any physical antecedent, and why may not the lens be one of the conditions of existence of such an antecedent? The case is however decisive on the only points on which it can furnish any evidence. First, the boy said that objects appeared extremely large. Franz's patient also "saw everything larger than he had supposed from the idea obtained by his sense of touch." Grant's patient likewise, on first seeing his guide, and being asked what idea he had of him before he saw, said he thought him "a much smaller machine, but of the same kind as himself." From this it is manifest that there was a positive visual perception of magnitude, that it was independent on that of distance, and that it was not a suggested idea of locomotion or of anything else. Observe there was no possibility of illusion; for as yet there was no association between the visual and any other perceptions. If the eye, therefore, conveyed no immediate perception of magnitude there could be no estimate of it at all until the knowledge of the object suggested the accustomed idea, which would then be equal to itself. Had it been said that the patients were able to make a good guess at the magnitude of objects, this, which would be surprising enough, might be explained away. But the fact that objects

nale dell' I. R. Istituto Lombardo," vol. xvi., 1847; Nunneley, "Organs of Vision," (1858), p. 31. These are all the cases I can discover in which the observations bear upon the question in hand. I have not been able to trace the original record of Grant's case, if, as I suppose, it was published in England. The account referred to is very meagre, and nothing is said of the previous condition of the patient.

as seen appeared much larger than they had expected, is a demonstration that what was presented was a new idea, and therefore a direct perception of sight. It is needless to remark, that if objects were seen as in contact with the organ they would appear small.

I attach importance to this apparently trivial fact, because in it we obtain the perceptions of sight pure and unmodified by those of touch, or by any association, and free from errors of observation. That it has not been noticed by any of the philosophers who have discussed Cheselden's case is doubtless owing to the prejudice that our estimate of magnitude is an inference from apparent distance. The statement was therefore set aside as anomalous and mysterious. But the coincident testimony of Dr. Franz's patient shows that we have here no accidental anomaly, but the evidence of a general and fundamental principle. This fact alone suffices to establish the conclusion—1°, that the object of vision is seen as external, and 2°, that it is seen as at some distance from the eye, and of some definite magnitude. But we may go even farther. For there is nothing to show that objects appeared to become larger or smaller, as the youth approached or receded from them; yet it is scarcely possible that he should not have mentioned so striking a phenomenon when speaking, as in several instances he did, of the apparent size of objects; there is even positive evidence to the contrary. When his father's picture in a locket was shown him he recognized a likeness, but was vastly surprised that it could be put into so small a compass; it would have seemed to him, he said, as impossible as to put a bushel into a pint. But, on the common view, the real face at a short distance must have appeared as small as the picture. If then we are justified in assuming that the apparent magnitude at different distances remained the same, we cannot avoid the conclusion that it was accompanied with a perception of the apparent distance.

Cheselden further notices, that "we thought he soon knew

what pictures represented which were showed to him, but we found afterwards we were mistaken; for about two months after he was couched he discovered at once they represented solid bodies, when to that time he considered them only as parti-coloured planes or surfaces diversified with variety of paint; but even then he was no less surprised, expecting the pictures would feel like the things they represented, and was amazed when he found those parts which by their light and shadow appeared now round and uneven, felt only flat like the rest, and asked which was the lying sense, feeling or seeing." The interpretation of this seems to be, that by this time an association had been established between the ideas of solidity, &c., and their pictorial signs, so that the latter now, for the first time, suggested the former. It follows, that the solidity which he had hitherto recognized by sight, was seen in a different way, and that the objects of sight had not appeared to be in a "coloured plane." If pictures had never conveyed to him the idea of solid figures, it is clear that solid figures were not seen as plane pictures. He had as yet had no reason to suspect a discrepancy between sight and touch.

A year after his recovery, being brought to Epsom Downs, and seeing an extensive prospect, he said that this was a "new kind of seeing." In this remark is implied a recognition of a new visual sensation. It was clearly not the suggestion of a great effort of walking which appeared to be a new kind of seeing; nor was any such suggestion possible on the first view of a distant prospect. This observation therefore proves that in distant vision there was a visual sensation quite distinct from that conveyed by a plane picture.

But, as already remarked, the perception of distance must be extremely imperfect where there is but one eye, and that without any power of adjustment. Now Cheselden's patient was subsequently couched of the other eye. If he had no

perception of distance by sight this ought to have made no change. What was the fact? "Being lately couched of his other eye, he says that objects at first appeared large to this eye, but not so large as they did at first to the other; and looking upon the same object with both eyes, he thought it looked about twice as large as with the first couched eye only, but not double, that we can anywise discover." Here again we have a perception of magnitude independent on association, and that after an association had been already established. The disproof which this fact affords of the association theory of single vision has been before noticed. What I now draw attention to is the increased magnitude of objects seen with both eyes. I infer, first, that the object was seen at once with both eyes, and seen therefore in the same definite place. But there is no trace of an increase in apparent magnitude in normal vision when an object is seen with both eyes; it is still determined by the retinal image combined with the apparent distance. But we have seen that an increase in the perceived distance is accompanied with an increase in the perceived magnitude. And this is the only explanation that seems admissible here. For want of the power of adjustment distance was seen imperfectly before; but now the organ had acquired a new means of perceiving it, and near objects are consequently seen at a greater distance and of a greater size.*

But the patient, we are told, said that objects appeared to touch his eyes, and even Sir W. Hamilton refers to this as a confirmation of Berkeley's theory. Yet if we do not make allowance for the difference between the ideas and consequently the language of a blind man and a seeing the state-

* This increase in the apparent magnitude was not noticed in other cases. The interval between the two operations was however unusually great in Cheselden's case—apparently more than a year. In other cases it was only a few days or weeks.

ment is really absurd. There is no sensation of touch when we see; the external surface of the eye being insensible to light has received no sensation; the organ which has received the impression of light is insensible to touch. But touch was the only mode of perception with which he was acquainted, or which he could conceive. Having then no experience of perception of distant objects, nor any idea of distance from the organ, it was inevitable that he should at first conceive any new mode of perception to be a mode of touch; and he was already familiar with the eye as the organ of vision. The absurdity of the expression in any other view was noticed by Smith and Stewart.* The latter remarks, that if the patient connected the sensation of light locally with the eye it must have been from pain, as in any other case the impression of light on the retina is not accompanied with the perception of the part of the body on which the impression is made.† Daviel, Janin, and Duval never noticed that their patients thought objects to be in contact with the eye. Mr. Nunneley states that his patient did so think, adding, however, that "he walked most carefully about with his hands held out before him to prevent things hurting his eyes by touching them." This would have been a very absurd proceeding if he thought these things already touched his eyes. The account of Grant's case is similar. On first seeing he asked for his guide, "But," said he, "I think I can get on without him." He then tried a few steps, but "everything seemed to make him afraid."

The observations of Dr. Trinchinetti on this point are more exact, and very instructive. He operated at the same time

* Smith, Essays, p. 315. Stewart, Works, vol. iv., p. 309.

† This is well illustrated by a case of Home's (the first), in which while the patient was blind if a lighted candle was held before him at a less distance than one foot he said it touched his eyes, but when it was moved further off, although the light was still perceived (up to twenty-two inches), he said it did not touch them. The sun also seemed to touch his eyes.

on two patients (brother and sister), eleven and ten years old respectively. The same day, having caused the boy to examine an orange, he placed it about one metre from him, and bade him try to take it. The boy brought his hand close to his eye ("quasi a contatto del suo occhio"), and closing his fist, found it empty to his great surprise. He then tried again a few inches from his eye, and at last, in this tentative way, succeeded in taking the orange. When the same experiment was tried with the girl she also at first attempted to grasp the orange with her hand very near her eye ("colla mano assai vicina all' occhio"), then, perceiving her error, stretched out her forefinger, and pushed it in a straight line slowly until she reached the object. Other patients have been observed (by Janin and Duval) to move their hands in search of objects in straight lines from the eye. Dr. Trinchinetti indeed regards these observations as indicating a belief that visible objects were in actual contact with the eye; but after what has been said it is hardly necessary to point out how completely they confirm the foregoing remarks, proving that the idea of contact did not belong to the visual perception as such at all, but to the form under which alone perception had hitherto been conceived. It is especially worthy of remark, that when the boy had missed the orange on his first attempt he sought to seize it at gradually increasing distances until he succeeded.

What this notion of objects touching the eye proves, is, not that the object of vision is seen at no distance, but that previously to the operation there had been no idea of distance at all. If touch had given a knowledge of distance, then on the acquisition of sight, since there is no sensation of touch in the organ, objects known to be at a distance would not appear to touch the eye. On the other hand, if the idea of distance is due to sight only, then, at the first opening of the eyes, the objects perceived must be thought

to be in contact, and the patient would have to learn, quickly no doubt, but gradually, to use his new sense. As Platner* well remarks:—"For the very reason that space and extension are empirically possible only through sight, one born blind has to learn to live in space." This view is very strongly confirmed by the fact that where there has been previously any perception of colour, this notion of contact with the eye does not exist. Thus Home's second patient being asked whether objects seemed to touch his eye, answered no; on the contrary, he moved his eye to different distances in order to find the best point of view. Yet before the operation he could only distinguish colours, and had no knowledge of outline. His former patient, on the contrary, possessed only this imperfect degree of vision after the operation, having had no sense of colour before. Accordingly, in this case objects did at first seem to touch the eye, until the second eye was couched (as it appears), when the "sun and other objects did not now seem to touch his eyes, but appeared to be at a short distance from him." Mr. Ware also concluded from observations on two children blind from birth, that their knowledge of colours was sufficient to enable them to tell whether a coloured object was brought nearer to the eye or further from it—whether for instance it was at two or four inches distance—nor had either of them the least impression that the coloured objects touched their eyes. In these cases he did not operate, but in another in which he did so, the results were very remarkable.

This patient was eight years old, and before the operation could distinguish the colour of an object held close to his eyes, if strongly marked, but on no occasion did he ever notice its outline or figure. After the operation, Mr. Ware held a letter before him, at a distance of twelve inches, when, after a short hesitation, he said that it was a piece of paper and square,

* "Philosoph. Aphorism.," (ed. 1793) p. 440.

which, he said he knew by the corners, but larger in one direction than in the other; he then pointed to the corners with the greatest precision. Being shown a small oblong bandbox covered with red leather, he said it was red and square, and pointed at once to the corners. A small oval silver box he said had a shining appearance, and presently added that it was round, and had no corners. When a white stone mug was shown to him, he said at first that it was a white basin, but corrected himself, and said it must be a mug, because it had a handle. The next day, being shown a table knife, he at first called it a spoon, but then corrected himself, and named it correctly, pointing to the blade and handle as he was desired. On Mr. Ware holding up his hand before him he knew it, but could not at first tell how many fingers there were, nor distinguish them from one another. Mr. Ware then showed him his own hand, and desired him to remark the difference between the thumb and the fingers, after which he readily pointed out the same parts in Mr. Ware's hand. Even on the first day he could always tell whether an object was moved nearer or farther from him; and indeed as soon as he was able to see, he had distinguished a table a yard and a-half from him, and said it was a little further off than his hand could reach.

Now, what was the previous history of this case? His parents stated that he had never distinguished any of these objects by sight, and when he wished to distinguish colours, which he could not do unless they were very strong, he had been obliged to hold the coloured object close to his eye, in fact almost touching it. He had not, however, been actually born blind, but had lost his sight before he was a year old. It cannot be imagined that an association had been then established between the varied sensations of sight and touch, with such force and exactness as to be preserved unimpaired during twenty-one years of blindness, and ap-

plied at once to determine the place and form of new objects. But the idea of space once acquired, had entered into all his conceptions of the external world, and therefore the patient on his recovery was not introduced into a new world; he had already learned to live in space. Another similar case is recorded by Mr. Ware, of a young man (fourteen years old) who as well as his parents affirmed that he had never seen the figure of any object; yet on the first trial after his recovery of sight, he took hold of Mr. Ware's hand when held at different distances, saying whether it was brought nearer or carried farther from him. In order to satisfy the observers of the accuracy of his perceptions, he conveyed his hand to Mr. Ware's in a circular direction. In this case also it is probable, although there was no recollection of it, that some notion of figure had been acquired before the blindness became complete. In the following interesting case, however, this supposition is excluded.

Mr. Wardrop's patient was much more completely blind before the operation than even Cheselden's. She could distinguish light from darkness, and knew the direction of bright sunlight; but in other circumstances could not tell the direction of the window. The pupil was in fact quite shut up, and no light could reach the retina except through the iris. Both eyes had been unsuccessfully operated on (probably for cataract) when she was only six months old, and one of them completely destroyed in consequence. The operation for artificial pupil was performed when she was forty-six years of age. Until the third operation she seems not to have been able to distinguish objects at all. Her first observation confirms what has been said of the visual perception of magnitude. When returning home after the operation, seeing a hackney coach pass, she asked, "What is that large thing that passed us?" The same evening she was

given her brother's watch, and examined it for a long time, holding it close to her eye. Being asked what she saw, she said, a dark side and a bright side, and asked further, she *pointed to the figure* 12 and smiled. Asked if she saw anything else, she said yes, and *pointed to the figure* 6 and *to the hands.* On the third day she looked at her brother's face, and said she saw his nose. He asked her to touch it, and *she did so.* He then slipped a handkerchief over her face, and asked her to look again, when she playfully *pulled it off*, and asked, "What is that?" On the eighth day she was able to walk across the room from a chair to the sofa, without assistance; and on the next day said to her brother, "I see you very well to-day," and *went up* to him and shook hands. She expressed great disappointment at being unable to combine the sensations of sight and touch, saying, "I cannot tell what I do see; I am quite stupid." How indeed could it possibly be otherwise? She appears also to have had great difficulty in directing her eye to an object, moving hand and eye in various directions, as one blindfold gropes about. Even at the end of forty days, it was only after many trials that she could direct her eyes to an object. Duval also notes the difficulty his patients found in directing their eyes to the object sought, turning the head right and left, up and down, as infants do. This is just what we should have expected.

We have here the direct and immediate perception of externality and positive distance, wholly independent on muscular exertion. This is implied for instance in the intentional interposition of the hand between the eye and the object, as in pointing to the figures of the watch. Mr. Nunneley's patient also put his hands purposely between his eyes and the objects. But even if she had been able to estimate distance accurately, she was necessarily ignorant

of the proper motions of the hand and eye, corresponding to certain visual perceptions. These are of course only learned by association, as has been already pointed out. Some writers indeed appear to suppose that the visual perception of distance implies a visual revelation of the amount and direction of the muscular exertion required to reach an object; but it is, I hope, unnecessary to expose any further the absurdity of this. In the case before us however, even a tolerably correct visual estimate of distance was out of the question, when the patient had but one eye, and that disordered and unused for nearly a lifetime. Yet we find even at the very first the power of directing the hand with some degree of accuracy to the object seen. Indeed the perceptions of distance, figure, and position, showed on the whole such perfection as might fairly suggest a doubt whether the lady had been born blind.* But this is placed beyond question by the history of the case. It is to be regretted that we have no means of judging whether

* Mr. Cowell, in his ed. of Berkeley's "Vindication," speaks of this case as strongly favouring Berkeley's views (p. 129), for what reason I cannot imagine, except because the lady could not identify the objects she saw with those previously known by touch. I have therefore italicised some of the most significant observations. I have not referred to the observations made with the express object of determining what ideas she had of distance, figure, &c. These were not made until the eighteenth day, and she was then able not only to distinguish and name correctly different figures, but to show the accuracy of her ideas by describing the like figures with her finger. Mr. Mill, who takes no notice of the previous observations, considers that in this interval she might have established the association between the visual signs of distance and the tactual perceptions. But as forty days' experience did not teach her the ordinary motions of the eye, this might well be doubted. However, I have thought it best to pass by these special experiments altogether, which moreover were not very well conducted. For instance, the two objects chosen to test her power by recognising by sight what she had just before felt, were a pencil case and a key!

the lens was present or not. If the operation undergone in infancy was for cataract, it is possible that the lens may have been reproduced, as frequently happens;* but the unused muscles could scarcely have retained the power of adjustment.

We are indebted to Dr. Franz for a history of a case in which a very intelligent young man was successfully operated on for cataract, and a series of experiments carefully instituted, which was published in the "Philosophical Transactions" twenty years ago (1841). It is remarkable as the case in which the previous blindness was the most perfect, the patient the best instructed, and the observations the most accurate; yet as far as British metaphysicians are concerned it might as well have been buried in the pages of the "Illuminated Doctor." It is ignored by one and all of them, and is only noticed (and that very imperfectly) by German philosophers; while Cheselden's inconclusive case has been sedulously copied by one from the other for more than a hundred years.

The young man to whom I refer was affected with congenital cataract and strabismus in both eyes. The right eye had no perception of light at all; the left was sensible to

* As the reproduction of the lens is a remarkable phenomenon, and scarcely alluded to in the common text-books, I subjoin a reference to the principal authorities:—Coiteau and Leroy d'Etiolle, in Magendie's "Journal de Physiologie," vol. vii. (1827); transl. in Froriep's "Notizen," vol. xvi.; Middlemore, "Lancet," 1828, and "London Med. Gaz," vol. x. (1832); transl. in Froriep's "Notizen," vol. xxxi., p. 297 (relates experiments on animals); Fronmüller, "Beobacht. auf dem Gebiete der Augenheilkunde," Fürth, 1850, pp. 62, 65. According to J. D. Forbes, the reproduced lens is not capable of adjustment ("Edinb. Trans," vol. xvi., p. 5). But Home found traces of this power in persons who had been operated on for cataract a year before; and from what we have seen above (p. 93) this was in all probability due to a reproduced lens. Fronmüller reckons nineteen recorded cases, the earliest having been observed by Vrolik.

light and colours, but only those of an intense and decided tone. The patient himself believed that he saw as much as one-third of a square inch of a bright object held within an inch or half an inch of the eye, and in a certain direction; but Mr. Franz was of opinion that he could not possibly have any perception of objects. He remarks, that all well-educated blind, who are not absolutely amaurotic, endeavour to persuade us that they can see more than they really do. At all events perception of figure and distance was wholly out of the question before the operation. Three days after the operation his eyes were uncovered, and being asked what he saw, said he saw "an extensive field of light in which everything appeared dull, confused, and in motion." He could not distinguish objects. When his eyes were again uncovered, after two days, he described what he saw as a number of opaque watery spheres moving about with the movements of the eye and partially covering each other. This appearance diminished daily, and he was able, as he said, to look through the spheres.

As soon as these had disappeared and he was able to look at objects, he was shown the outline of a square, within which was a circle, and within this a triangle. After careful examination he recognised the figures, and described them correctly. When asked to point out either figure, he moved his hand as if feeling, and with caution. Being shown a cube and a sphere, he said he saw a quadrangular and a circular figure; and after some consideration pronounced the one a square and the other a disc.

It is instructive to compare this result with the observations on Home's second case. This patient being shown square, circular, and triangular cards in succession, at first called them all alike—"round." Being afterwards shown a square and asked could he find any corners to it, he wanted to touch it, but being told he must not do so, he examined

it some time, and said at last that he had found a corner, and then readily counted the four corners; and subsequently the three of a triangle. Even thirteen days later he could not tell the shape of the same cards without counting their corners, but after another week he did so readily.* In comparing these two cases, we are struck with the superior quickness and accuracy of Dr. Franz's patient; and it appears unquestionable that this was owing to the superiority of his previous education, in other words, to the superior development of the ideas of touch. This is in exact accordance with the conclusion above arrived at (p. 144). The cube and sphere used in the experiment, were each four inches in diameter, and placed at a distance of three feet, at which distance the "accommodation line" of a normal eye is about two-and-a-half inches. Even if the young man's eye had been perfect, then, the bodies were too small and too distant for a fair experiment on the perception of solidity. But without a lens it would be doubtless impossible to recognise such figures, even after long experience. This young man was next shown a pyramid, which appeared to him triangular, but when it was turned about a little, so as to present two of its sides to view, after considering it a long time he said it was an extraordinary figure, neither triangular, quadrangular, nor circular, and at last said, "I must give it up." When asked to describe the sensation produced by the different objects, he replied that he had seen a difference between the sphere and cube at once, *and that they were not drawings* (in other words, he perceived that they had three dimensions), but had not been able to form from them the idea of a square and disc until he perceived a sensation of what he saw in the points of his fingers, as if he really

* If the reader will hold a card of even four or five sides 20° or 30° out of the direct line of vision, he will probably find that he cannot tell its shape correctly without a similar examination.

touched the objects. The reader will not fail to note this striking example of the law of Reproduction, insisted on in p. 17. When the three bodies were put into his hands, he was surprised that he had not recognised them by sight, as he was well acquainted with these solid mathematical figures by touch.

All objects appeared so near that he was sometimes afraid of coming in contact with them. A decisive proof that they were perceived to be at some distance. If he wished to form an estimate of their distance from himself or from one another, he examined them from different points of view, by turning his head from right to left. This is just the way in which chiefly Mr. Wheatstone supposes solidity must be perceived by persons with but one eye, who obtain by this means two slightly different perspectives in succession.

I have already noticed the remarkable statement of this young man, that everything appeared larger than he had supposed from the idea obtained by his sense of touch; but another very important circumstance in connection with the question of apparent magnitude is recorded by Dr. Franz. When shown a picture he could not be made to understand why a distant house was represented as no larger than a man in the foreground. He did not understand the meaning of the picture as a whole, though each part separately was clear to him. But he found no such difficulty in nature. This remarkably confirms what was observed above with reference to perceived magnitude. Something may be said then in defence of those ancient painters who, like the Chinese, neglected perspective. Although they fail to suggest distance, they represent the magnitude of near objects as it appears to the eye.

Combining these facts with the statement made by this young man, almost at the moment of his beginning to see, that he saw "an extensive field of light" we could not have

a more decisive proof that "the perception of an extensive field of vision distinct from the retina, and hence of distance and magnitude," is not, as Mr. Mansel asserts, "an acquired perception." That objects appeared flat is not surprising. It would be surprising if they had appeared otherwise at first sight, and to a single eye. It is by considerable distances and by repeated observation that the power of perception ought to be tested in such cases. A remarkable proof, in addition to those already noticed, that this gentleman could perceive distance is his remark, that "he was always obliged to bear in mind that the looking-glass was fixed against the wall in order to correct his idea of the apparent situation of objects behind the glass."

The following fact is also worthy of notice as illustrating the comparative slowness with which such associations are established between the ideas of different senses. "Though he possessed an excellent memory, this faculty was at first quite deficient as regarded visible objects; he was not able to recognize visitors unless he heard them speak till he had seen them very frequently. Even when he had seen an object repeatedly he could form no idea of its visible qualities in his imagination without having the real object before him."

Cunier's case is very briefly related; but is interesting, on account of the patient being an idiot. He could never educate her sense of sight; yet she readily seized fruit, sugar, bonbons, bread, &c., and saw them at the same distance as other persons. But she had no notion of distance, except when her passion for sweetmeats was excited; and hence she constantly stumbled against obstacles in the street or elsewhere, and several times fell down stairs.

Mr. Stafford's patient was also of weak intellect and about the same age (23). The early observations only show the slow development of her sight. She did not distinguish

objects at all until the third week; in a month she could distinguish white and dark, and in five weeks various shades of colour. At this period, it must be observed, her eyes were only uncovered weekly. Her power of measuring distance was not tested for nearly six months. She was then taken to different parts of a large ward, and asked the measurement from one part to another, which she gave with great accuracy; but she was unable to say how she had acquired the knowledge.

In all these cases we have evidence of the perception of an extensive field, which for that reason cannot have appeared to be in contact with the organ, but must have been perceived as at some distance. Even those who said that objects touched their eyes showed in the clearest manner by their actions that this was only a form of expression which indicated the limitation of their previous conceptions. We find also in general a power of recognizing figure depending on the previous education of the sense of touch. And lastly, we have even clear indications of the power to distinguish between solid bodies and paintings or plane figures. Now let it be remembered that, according to the observations of Platner, a person born totally blind has absolutely no conception of objects coexisting separate from (or out of) one another in space; and it will be admitted that these results indicate even greater perfection and readiness in the visual perceptions than we could have ventured to anticipate. It is true that Platner's conclusions cannot be applied without reserve to those blind persons who have any perception of colour, as most of those operated on for cataract had. But if any superiority does exist in such cases, it is a strong confirmation of our argument, since it can only be attributed to the possession of the elementary ideas of sight. With respect to Platner's doctrine, such cases combined with those of total blindness would furnish the clearest "instantia

crucis," inasmuch as where there is no power of distinguishing objects, any ideas of distance &c., which may be present cannot possibly be referred to association. I am not aware that total insensibility to light has ever been cured; but if we have not the "instantia crucis," we have the "instantiæ variantes;" for we find that where the perception of colours before the operation was least, or none at all, the ideas of space, &c., after were also most imperfect, and *vice versa.* Any deficiency in these instances is more than supplied by the cases in which sight had existed for a very short period in infancy, and in which we find the perceptions of figure and distance amazingly clear and perfect.

We are obliged to conclude from the preceding histories that the perception of distance (as distinct from the measure of it) is not dependent on the power of adjustment, but is the natural concomitant of vision; and this is in accordance with the conclusion at which we had already arrived. It is strikingly confirmed by the case cited from Home, in which the patient while blind imagined that a candle touched his eyes if it was nearer than twelve inches, but not when it was removed further; for there was here a notion of distance obviously independent on touch or the motion of the eye.*

* Volkmann expresses his regret that we are not informed what were the first ideas of beauty and deformity in these cases; whether such objects as we esteem beautiful appeared so to persons newly gifted with sight. It may not be amiss then to mention one or two observations which I have noted touching this question. To Trinchinetti's patients all faces seemed beautiful, and on being shown pictures of human faces beautiful and ugly they preferred the more beautiful. These, "like all others," he remarks, admired the beauty of flowers especially when large and bright-coloured. Dr. Franz's patient was more pleased with the human face than with any other object; he thought the eyes most beautiful, especially when in motion, but the nose disagreeable. A boy (eleven years old) mentioned by Duval, having by chance seen a mirror, contemplated the image with pleasure, examining the mirror all round, and especially behind, and became "ivre de joie" when told that it was a

CHAPTER XI.

OF THE EVIDENCE DERIVED FROM INFANTS AND ANIMALS.

I PROCEED to consider the phenomena exhibited by infants and by the lower animals. The case of infants need not detain us long; they have in some respects indeed the advantage over the blind, since an instinct or faculty unemployed may tend to decay, but in the infant all the natural powers are fresh. Nothing however is more difficult than to trace the earliest development of ideas in the mind of an infant. But if the supposed association between the ideas of sight and touch exists at all it is in early infancy it must be established. We ought therefore to find touch, the sense first developed, constantly appealed to, and most fruitful of information; and, on the other hand, we should see manifold traces of the imperfection of vision. Vision must indeed in any case be imperfect, since the organ is not perfectly developed at the time of birth. The results however of careful observation may be stated briefly as follow.

In the first instance, the vivid impression of light from a

representation of his own face. Of Cheselden's patient we only learn that he had some independent idea of beauty; he wondered that the things and persons he liked best did not seem most beautiful. The only special fact mentioned is, that the first sight of a negro woman caused him horror. Indeed the first time he saw black it gave him great uneasiness. Bright colours generally please, but they frightened the idiot girl in Cunier's case. The first time Mr. Wardrop's patient walked out, and saw the clear blue sky, she said, "It is the prettiest thing I have ever seen yet, and equally pretty every time I turn round and look at it." In all these observations there is a striking analogy to what may be remarked in the case of infants.

candle exercises indirectly a motive, or rather at first a resisting force, so that when the eyes are fixed on the light the body may be turned through a considerable angle without the head moving, as if the axis of vision were a rigid bond. Next (at three weeks) the light and other objects when distinguished are sought by the motions of the head. This might be considered to be consistent at least with the notion of contact; but soon afterwards the eyeball itself is turned, and in the seventh week objects are not only followed with the eye without any motion of the head, but are sought, and when seen sideways the eye is turned towards them. This is already inconsistent with the notion of contact. When objects, faces, &c., begin to be recognized it is at first only at short distances of one to three feet, but gradually they are known at three or four yards (in fourth month). The light of a candle is of course observed from the first at a much greater distance. Now it is easy to see that the convergence of the axes even in the third or fourth week is suitable to the distance of the object, and as objects are discerned at greater distances the convergence changes regularly and rapidly with the distance. This is very manifest in the third month. Soon after the infant has learned to distinguish the faces of his friends he makes also motions with his body, hands, &c., as if trying to come towards them;* he knows when a friend or stranger takes him in his arms, "turns away from the latter and clings to the former," looks for them when they retire, and in short manifests in every way that he can his perception of their externality and distance. At five or six months (to fix the latest period) he shows, as clearly as it is possible to do, his appreciation of degrees of distance. For instance, when he makes futile efforts to reach a distant object, he shows by signs of satisfaction or the contrary that he knows when he

* Infants born blind apparently never stretch out the hand to catch at anything. Home, Philosoph. Trans., 1807, p. 84.

is moved towards it or away from it; nor does he attempt to seize it until brought quite close. Of course he has all this time been establishing associations between the ideas of touch and sight, between signs of distance and efforts of motion, &c. Nothing of the kind. All this takes place before the infant has learned even to touch things with his hand at all, much less to reach for them; nay, at a period when he has not learned his very first lesson in the motions of the hand—namely, to bring it to his mouth (which he begins to do with accuracy about the eleventh week), and long before he has learned to know his own hands as objects of sight, *i.e.*, to associate his own motions with those of these particular external objects. In fact, his first motions towards a desired object are not of the hand, but of the head. Even at four, and up to six months, his attempts to seize objects are of a tentative character, often unsuccessful, and resembling those described of persons born blind; and for some time after this, and when near objects are seized with tolerable accuracy, the motions of the hands are of a rough, irregular, and wild character, as far as possible from being adapted to convey definite ideas of distance or effort, or to be associated separately with definite visual sensations.

Of course as long as he is unable to walk he can have no idea from locomotion of any distance beyond the few inches which he can reach with his hands. If sight then gives no independent idea of magnitude, he must see his mother or his nurse at one time appearing the size of a doll, while at another her face fills the field of view, and again passing from this appearance to the former with prodigious velocity. How is it possible that he could recognise her as the same person in these different circumstances? What total confusion must be produced in his mind! We may observe, on the contrary, that at the same time that a strange face at some distance causes alarm or perplexity, a doll or another

child seen under the same visual angle causes only amusement. In a word, the infant shows in every possible way that the development of his power to perceive and measure distance by sight, keeps pace even from birth with his general growth in intelligence; while the powers of voluntary motion are developed much more tardily. Even these, however, precede the manifestation of touch proper. We have every reason therefore to conclude that there is an independent visual perception of distance and magnitude in the earliest infancy prior to any association with touch or locomotion.

I may cite the testimony of two eminent and unprejudiced writers, one a physiologist, the other a philosopher, to the general accuracy of the preceding remarks. "At a very early period," says Schroeder van der Kolk, "the infant can bring his little hands to his mouth; subsequently in the third month he catches at an object to endeavour to raise it to himself; proper touching and handling succeed much later, and demand a higher degree of mental activity and special investigation. Hence the absurdity of some writers that the child receives the first impressions of distance and size by the touch, and by feeling learns to see. On the contrary he sees and distinguishes objects at various distances long before he seizes them in his hands and begins to examine them."*

Adam Smith also, although a strenuous supporter of the Berkeleian doctrine, remarks :—"Children appear at so very early a period to know the distance, the shape, and magni-

* "On the Independence of the Soul," translated in Winslow's Journal of Psychological Medicine, July, 1860, from the Album der Natur. There are but few observations on the mental life of infants published. Some scattered remarks may be found in Waitz "Grundlegung der Psychologie," p. 97 [Burdach, Physiol., vol. iii., p. 260], F. Carus "Psychologie." Nachgelassene Werke, vol. ii., p. 46; Scheidler Art. "Gesichtsinn" in Ersch and Gruber's Encycl., p. 187, after Schulze [Psych. Anthropol., p. 98].

tude of the different tangible objects which are presented to them, that I am disposed to believe that even they may have some instinctive perception of this kind, though possibly in a much weaker degree than the greater part of other animals." "A child that is scarcely a month old, stretches out the hands to feel any little plaything that is presented to it. It distinguishes its nurse, and the other people who are much about it from strangers. It clings to the former and turns away from the latter. Hold a small looking-glass before a child of not more than two or three months old, and it will stretch out its little arms behind the glass, in order to feel the child which it sees, and which it imagines is at the back of the glass. It is deceived, no doubt; but even this sort of deception sufficiently demonstrates that it has a tolerably distinct apprehension of the ordinary perspective of vision which it cannot well have learnt from observation and experience."*

We now come to the case of the lower animals, not to find a difficulty, an anomaly, but a confirmation, an analogy. Let it be remembered that the sole positive argument relied on by the Berkeleians is the physical impossibility that distance should be perceived by the eye. Now if animals whose eyes are constructed on precisely the same principle do perceive distance directly, this fact appears to overthrow the argument completely. There is, indeed, no instance in which we are more justified in reasoning from the lower animals than the present. Sight is the most universal special sense. The organ is in all vertebrate animals extremely uniform in construction, and even in the invertebrate the chief difference is in the substitution of an optical arrange-

* Essays, *ubi infra.* Does he state this last fact from his own observation? As it tells against his own doctrine, we may suppose that he had it at least on good authority. But one would like to know the subsequent history of this precocious infant.

ment for the voluntary motion of the eye. The order of certain parts of the retina is also changed, but their structure remains essentially the same. All the higher vertebrate animals move the eye in the same way, and so far as we can judge have a similar power, though with different limits, of distinguishing the form and distance of objects. There is a difference as to the convergence of the axes, but as we know nothing of the conditions of correspondence of the two retinas in animals, we must leave this out of consideration. Now the question before us is, what is the origin of this power? If we find that in animals it is congenital and therefore belongs to the organ of sight itself, it becomes in the highest degree improbable that in man it does not belong to the same organ, but is the result of a different faculty, the gradual product of association. The greater the resemblance between the organs of sight and the general phenomena of the nervous system in men and beasts, the greater this improbability. As every advance in physiology tends to develop this resemblance and to strengthen the conception of the unity of system in the animal kingdom, this argument has also become stronger.

Now that most animals antecedently to experience have some such instinctive perception as that in question seems abundantly evident. The moment the chicken has broken the shell it will dart at and catch a spider. Sir Joseph Banks said he had seen a chicken catch at a fly whilst the shell stuck to its tail.* "The hen," says Adam Smith, "never feeds her young by dropping the food into their bills, as the linnet and the thrush feed theirs. Almost as soon as her chickens are hatched, she does not feed them, but carries them to the field to feed, where they walk about at their ease, it would seem, and appear to have the most distinct perception of all the tangible objects which surround them.

* Bell on the Hand, p. 255 (ed. 1852).

We may often see them accordingly by the straightest road run to and pick up any little grains which she shows them, even at the distance of several yards; and they no sooner come into the light than they seem to understand this language of vision as well as they ever do afterwards. The young of the partridge and of the grouse seem to have at the same early period the most distinct perceptions of the same kind. The young partridge, almost as soon as it comes from the shell, runs about among the long grass and corn, the young grouse among long heath; and would both most essentially hurt themselves if they had not the most acute, as well as distinct perception of the tangible objects which not only surround them but press upon them on all sides. This is the case, too, with the young of the goose, of the duck, and, so far as I have been able to observe, with those of at least the greater part of the birds which make their nests upon the ground, with the greater part of those which are ranked by Linnæus in the orders of the hen and the goose, and of many of those long-shanked and wading birds which he places in the order that he distinguishes by the name of Grallæ. The young of those birds that build their nests in bushes, upon trees, in the holes and crevices of high walls, upon high rocks and precipices, and other places of difficult access; of the greater part of those ranked by Linnæus in the orders of the hawk, the magpie, and the sparrow, seem to come blind from the shell, and to continue so for at least some days thereafter. Till they are able to fly they are fed by the joint labour of both parents. As soon as that period arrives, however, and probably for some time before, they evidently enjoy all the powers of vision in the most complete perfection, and can distinguish with most exact precision the shape and proportion of the tangible objects which every visible one represents. In so short a period they cannot be supposed to have acquired those powers from experience, and

must therefore derive them from some instinctive suggestion." "The young of several sorts of quadrupeds seem, like those of the greater part of birds which make their nests upon the ground, to enjoy as soon as they come into the world the faculty of seeing as completely as they even do afterwards. The day or the day after they are dropt, the calf follows the cow, and the foal the mare, to the field; and though from timidity they seldom remove far from the mother, yet they seem to walk about at their ease, which they could not do unless they could distinguish with some degree of precision the shape and proportion of the tangible objects which each visible one represents." "The young of other quadrupeds, like those of the birds which make their nests in places of difficult access, come blind into the world. Their sight however soon opens, and as soon as it does so they seem to enjoy it in the most complete perfection, as we may all observe in the puppy and the kitten. The same thing I believe may be said of all other beasts of prey, at least of all those concerning which I have been able to collect any distinct information. They come blind into the world; but as soon as their sight opens they appear to enjoy it in the most complete perfection."*

F. Cuvier also remarks, that although animals cannot, as man does, feel objects in all directions so as to ascertain their form, they can guide themselves from the earliest moments of their life. "I have brought together," says he, "many observations which have proved to me that in a great number of cases this phenomenon is instinctive, for many of these animals on coming into the light see at once objects outside their eyes, and even at their real distance; they flee from them, avoid them, and conduct themselves in reference to them as if a long usage had perfected their experience."†

* Essays on Philosoph. Subjects, p. 318, sqq.

† Mém. du Muséum d'Hist. Nat., vol. x., p. 257. Stewart's Works, vol. iv., p. 379.

Of a young bison he remarks that "he was hardly born when he rose on his legs, and went almost running over every part of his stable without stumbling, and guiding himself as if he had known the places by experience."*

Phenomena such as these appeared to Smith and Hamilton all but decisive; but Mr. Mill will not yield so easily. "If," he says, "in a brute, as in a man, it be a necessary condition of vision that an image corresponding to the object should be formed on the retina [which he seems by no means prepared to admit], then there will be no resource either in man or beast for judging remoteness, except from difference in the degrees of brightness and of visible magnitude, and the only doubt will be whether these natural signs are interpreted instinctively or by virtue of previous experience." It is hardly necessary at this stage of the argument to point out the fallacies contained in this passage. It has been shown that the question of instinctive interpretation of signs is no more involved in that of the perception of distance than of any other perception. Animals and men would be ill provided indeed if brightness and visible magnitude were the only means of perceiving distance. Instinct is only a name for the unknown source of certain actions which, although not independent on the will, appear from their uniformity, &c., not to proceed from choice or conscious purpose. Because we give these a common name we are tempted to think that the word represents some positive notion. But it is simply equivalent to "impulse," and the expression as originally used was "natural instinct," or "natural impulse," indicating no more than that the action so spoken of proceeded from the nature of the animal. To speak of an instinctive perception in any distinguishing sense is absurd. Some animals may have powers of perception which we have not, but in all equally the perception must have for its occasion

* Flourens "De l'Instinct" (Paris, 1845), p. 135.

some organic affection, and when the proper affection is present the perception takes place in all alike involuntarily, and by an unknown connection.*

It is possible indeed that man should be obliged to learn by experience what animals perceive immediately; but no one has attempted to show that this is the fact in the case before us. The argument of the Berkeleians is, that it is physically impossible with such an organ as ours to see distance; and when it is shown that animals with the same organs do see it, then we are told that this is not perception but instinctive interpretation of signs, and this for no other reason, as it seems, than that the sensation is not like the perception. Indeed when we consider what the signs are which Mr. Mill has in view, there appears good reason to distinguish this interpretation from perception; but then there is the same reason to deny its adequacy to account for the phenomena. It may be asked, however, at least with regard to magnitude, when an instinctive interpretation of signs is spoken of, what is supposed to be the thing signified? Not the tangible magnitude; for with few exceptions animals have no special organ of touch, nor do they seem to possess it at all as an objective discriminating sense, except in the tongue. They have no power of ascertaining the magnitude or form of any tangible object, and their only idea of magnitude must therefore be a visual one. But it is manifestly as constant and correct in the dog and the hawk as in the monkey or the elephant. Sir W. Hamilton stated the question correctly when he said,† "If in man

* "We designate by the name instinct the operation of the soul as a natural power, bound to unchanging laws, and we oppose it to the conscious actions of the spirit in the adult man, as necessity to freedom." "Instinctive motions are such as the organism can order, not without the influence of the will and the co-operation of the soul on the nerves, which yet are accomplished without conscious will, without a conception of the object."—Waitz, "Grundlegung der Psychologie," p. 158.

† Lect., vol. ii., pp. 181, 182.

the perception of distance be not original but acquired, the perception of distance must be also acquired by them [the lower animals]." We now see that the phenomena exhibited by them are in accordance with the conclusions to which we have been led by a more accurate study of the laws of vision in man, and they therefore afford an additional confirmation of them.

CHAPTER XII.

CONCLUSION.

The first business of the philosopher is to ascertain facts before he devises theories to account for them. Now the facts in the case before us are—first, that objects of sight appear to be external; and secondly, that within certain limits we can discern their distance with great accuracy. On examining this power more closely, we find that we may conveniently distinguish two classes of distances. The distance of very near objects is given by sight with extreme accuracy; but of the more remote distances we judge but vaguely, and are liable to be deceived by various visual signs which do not influence us in the former case, in which, moreover, the perception has a peculiar vividness. Now the obvious conclusion is that in the former case there is some cause in operation which is absent in the latter. But philosophers on the contrary have inferred that because in great distances we are deceived by such and such signs, therefore these are the sole occasion of our perceptions even in those cases where they are insensible and the effect more striking. Having to account for a certain phenomenon, they examine those cases where it is manifested in the lowest degree, and conclude that the antecedent, which is there most conspicuous, is not only a condition but the sole one.

According to the current doctrine there are three or rather four moments in the perception of distance by sight. There is the visual impression or apprehension of the above-mentioned signs, the tactual perception, the original association,

and the process of suggestion. Any inaccuracy in any one of these must produce an equal inaccuracy in the result. But we have seen that each of these is highly uncertain and fallacious, and yet the result is far more accurate than any one of them. The uncertainty of the association in particular, is chiefly owing to the fact that the supposed perceptions of touch differ only in quantity, so that each is reached by passing through all below it. Hence each visual sensation cannot be associated exclusively with the appropriate tactual perception; on the contrary it is invariably connected with the beginning of the effort not with the end, which again is accompanied with a different visual sensation. This indeed is more than an occasion of error, it renders the association impossible, except on the supposition of an idea or perception of distance prior to the effort, which may serve as a link between the two otherwise separated perceptions. Supposing the association possible, the suggestion would introduce a further inaccuracy, since the memory of ideas of quantity and of degree is very imperfect. We have also seen that the perception to be accounted for does not correspond with that of which it is supposed to be a reproduction, either in its range or its variations, either in the conditions of its perfection or of its errors. For greater distances, for example, it is much less accurate; for near intervals, vastly more so. And this is just the reverse of what might be expected from the sensations supposed to be concerned.

On the other hand, we have found that the conditions of the perception in question, its range, errors, and perfection, do correspond with the utmost accuracy with the laws and conditions of certain unfelt and involuntary motions in the eye, which have been shown to be determined exclusively by visual sensations. What these sensations are we cannot indeed affirm positively, but we know that in their case there can be no question of association, since when an object

is distinctly seen and attended to they have already produced their effect as regards that object, and have ceased to be. We know also that the effect of which they are the occasion has an absolutely invariable relation to the distance of the object; and this is moreover the sole sensible phenomenon, with the exception of the accompanying perception, in which such a relation can be traced. Since the motion (which is indirectly dependent on the will) is neither the cause nor the effect of the perception, the conclusion is irresistible that they are effects of the same cause, and that therefore the perception of distance is the direct result of the visual sensations, which thus prove themselves adequate to such an objective perception. And in those cases where nature eliminates experience and intelligence from the previous conditions, we have just seen that the result accords perfectly with this conclusion.

Indeed, without having recourse to these exceptional cases, we have already the means of observing the perception in its most independent form—namely, in imagination. Suppose we try to imagine a definite length, surface, or distance, say an object a foot square, at the distance of a yard, is it a certain amount of touching and of arm-effort that we imagine? Not at all. Both the magnitude and the distance are conceived simply as objects of sight. There can be no question here of any influence of associated visual sensations; for the fact is, there is no definite visual sensation imagined at all, whether of brightness and distinctness or of muscular adjustment, all of which we may imagine to vary as we please. The perception is in fact given absolutely independent on any sensation; yet we find that we cannot imagine either distance or magnitude except as something seen, while we can dispense altogether with the ideas of touch and locomotion; indeed the difficulty is to bring them in. This is a decisive proof that the perception is a visual

perception, and that the ideas of motion, &c., are only suggested.

Indeed the sense of extension is quite distinct from that of colour, and it may or rather must be distinguished as a special sense. But if so, this last consideration alone is sufficient to prove that its external organ is the eye.* It would in truth be a singular anomaly in psychology if it were otherwise, since sight is the only sense whose sensations involve the notion of space or extension as their indispensable form.

But sight is also the only sense which conveys any knowledge of objects of which our own body is physically independent. Touch is limited to those few objects which actually affect us mechanically, and which would equally obstruct our motions whether we felt them or not. The eye can receive impressions at once from objects within a finger-breadth and as remote as the stars, objects on which our motions are independent. Hence, while touch gives us words expressive of the feelings and emotions, and furnishes most of the terms of æsthetic science, every word indicative of original objective knowledge whose history can be traced has been borrowed from sight. That the sense which alone can apprehend the distant or the separate should also be the sense properly perceptive of distance is fitting and natural; but that it should indirectly borrow its estimate of distance from a sense for which the distant has no existence, would be strange and anomalous. The anomaly would be increased when we consider that the perceptions of touch are, as it were, fettered to the organ in which we feel a sensation,

* In reference to this, it is interesting to remark the connexions of the optic nerve. According to Panizza, it is traceable to several origins: besides the corpora quadrigemina, to the thalami optici, and also to the fibrous bundles from the posterior convolutions.—Giornale dell' I. R. Istituto Lombardo, vol. vii., (1855), p. 237.

whereas in sight there is no local sensation whatever, and the sense is therefore forced to place its object in an outer world, but free to place it at any distance therein. If there be any direct perception of distance at all it can belong to no sense but sight; that it does belong to it has I hope been proved, although much remains to be done before the antecedent physical conditions can be stated positively.

DUBLIN: Printed by ALEXANDER THOM, 87 & 88, Abbey-street.